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ART. I.—*The Effect of a Warmer Climate upon Glaciers;* by Capt. C. E. DUTTON, U. S. Ordnance Corps.

INQUIRY into the possible causes which could have produced a great extension of glaciers has been involved in the following difficulty. While it cannot be doubted that the climate of the Glacial period was in some important respects different from the present climates the moment any attempt is made to ascertain what would be the effect, if any one of the determinants of climate were to undergo a marked variation, it is found to be so intricately interwoven with many other conditions and determinants that the ingenuity of the investigator is generally baffled in his endeavors to assign a just and proper weight to them all. The intricacy of the problem is very great and its difficulties are analogous to those which would beset the investigator who should endeavor to ascertain what would be the effect of altering the size or adjustment of any one of the parts of a very complicated machine of which the structure and operations were very imperfectly understood. Apart from this intrinsic difficulty, which is a sufficient excuse to any investigator who may have failed to reach satisfactory conclusions, there has been on the part of some inquirers a want of care and an inaccuracy of method which is not so excusable. Some of them have ventured to offer conclusions as to what would be the result of modifying one part of the machine without troubling themselves to inquire what coördinate modifica-

tions would be required by the other parts, or even whether any such modifications were at all necessary. A conspicuous instance of this faulty method is furnished by those who argue that in order to account for more extended glaciers than we now have we must infer that a more copious snowfall prevailed in the Glacial period; that to provide this more copious snowfall we must infer that the air was more moist, the evaporation greater and the temperature of the atmosphere at large higher than now; in brief, that the climate of the earth was then warmer than at present; probably by reason of a greater rate of solar radiation. The questions which this hypothesis raises are much more limited and less complex than those brought up by other theories of a glacial climate, though even here the complexity is considerable. I believe that it can be brought to the test of a decisive argument founded upon known laws and relations, and to a conclusion which will not admit of any dispute. In that conviction the following argument is submitted:

(1.) It is agreed on all sides that glaciers can form only, and will always form when, the annual snowfall is for a long term of years greater than the annual dissipation of snow by liquefaction and evaporation. It is taken for granted that nobody will question the assumption that a warmer climate will increase the annual liquefaction and evaporation. It remains to inquire whether it will also increase the snowfall; and increase it to an extent which shall more than compensate the increased dissipation.

(2.) It is sufficiently obvious that the amount of snowfall in any given locality in one year will depend upon the two following quantities: 1st, the number of days or hours during which snow is falling; or, more simply, upon the *time* of precipitation; 2d, upon the average *rate* at which it falls. The product of these two unknown quantities will express the total quantity of snow during any period. They are sufficiently general to cover all possible conditions which can affect the amount of snowfall, and the question is thus resolved into the subsidiary ones: 1st, will a warmer climate cause in any locality any increment in the time of snow precipitation in an average year; 2d, will it cause any increase in the average rate? These will, so far as practicable, be considered separately. It will be most convenient to examine, first, the rate of precipitation. But before doing so it will be well to advert to two or three familiar but most essential facts. They are truisms, indeed, but are necessary for the continuity of the argument.

(3.) Precipitation takes place when saturated air is cooled; never otherwise. If the two limiting temperatures between which cooling takes place are both above zero (centigrade) the

precipitation is necessarily all rain; for the precipitate must take the temperature of the menstruum. If the two limiting temperatures are both below zero the entire precipitation is snow. If one limit is above and the other below zero a part of the precipitation is rain and the other part snow. From this it follows that *whatever moisture air may contain in excess of the quantity which is necessary to saturate it at zero can fall only as rain; and the only available supply which can form snow is a portion of the moisture which is required to saturate air at zero.*

(4.) To avoid circumlocution it will be considered, unless distinctly specified otherwise, that the air is saturated with moisture at all temperatures. The amount of precipitation is an increasing but complicated function of the amount of fall of temperature. Without such a fall there can be no precipitation. But equal falls do not give the same amount of precipitation in different parts of the temperature scale. Thus a fall from 20° to 19° precipitates much more than a fall from 10° to 9°, and this much more than from 0° to −1°. The exact form of the function representing the relation of precipitation to equal heat-changes is not known, but it may be given very approximately at practical temperatures by the ordinary empirical curve of saturation used as a graphic representation. In reality, the amount of precipitation is the integral product of two variables, one expressing the amount of heat-change (or number of degrees fall of temperature), the other expressing the part of the temperature scale in which the cooling occurs.

(5.) In proceeding to investigate the questions formulated as above we are met at the outset with a difficulty which must be at once disposed of. This is the extreme irregularity of precipitation not only in different parts of the year but as between different years. In most places not one day in four is wet. The intervals of dry weather vary from a few hours to months. The storms are of the most unequal duration and copiousness. In any two consecutive years there is no correspondence in the calendar dates on which the storms occur, nor is the yearly or monthly or seasonal precipitation the same. How then can there be said to be any such thing as a rate of precipitation, or any rationally expressible time of snowfall? The answer is, that a great glacier is the work of centuries of accumulation. These factors, seemingly so lawless and so heterogeneous from day to day and from year to year, and so frequently altogether discontinuous, have, in the course of many centuries, each an average, and these averages are susceptible of perfectly rational expression. Suppose we had thrice-daily observations for a thousand years of all the meteorological conditions of a locality. The average of these observations would, in respect to each factor, give perfectly definite values and relations. Let us

therefore consider an imaginary year which shall represent the average conditions of a thousand years. It would have such characteristics as the following: In most localities every day in the year would show some precipitation, but with one or more maxima in one or two parts of the year, and a corresponding number of minima. In a few localities it is just possible that some part of the year might show no precipitation at all, the function becoming discontinuous. Each day in the year would have some definite rate of precipitation (quantity divided by the time). Each day would have its proper seasonal temperature and variations of temperature and all would follow some expressible law determined by the general and local conditions operating upon the climate of the locality in question. It is just these averages which determine the annual rate of growth or decline of a glacier in any locality, or whether there shall be any glacier there at all.

(6.) Let us now proceed to consider the effect of a change of the amount of solar radiation upon rate of precipitation—not snowfall just now, be it observed, but the entire precipitation both rain and snow. We shall separate the snowfall afterwards. This is quite necessary, for the omission to consider the total effect of a general increase of climatal temperatures has been the stumbling-block of quite a number of those who have appealed to it.

The *rate of precipitation* (i. e. quantity of precipitation per unit time—which time-unit may be taken very small, say one hour, or even less,) depends upon three factors: 1st. The quantity of air which undergoes cooling; 2d. Upon the rate of cooling; 3d. Upon the temperature at which the cooling begins. These three factors involve, either explicitly or implicitly, all known conditions which can possibly affect the rate of precipitation, except physical constants, which, of course, need not be considered. Thus the *amount* of cooling, i. e. number of degrees fall of temperature, is implied in the second factor; for this amount is simply the rate multiplied by the time, and the time is now unity.

(7.) The quantity of air which at any time is yielding moisture to any locality is that quantity which blows over it as wind. If the atmosphere were always motionless its hygrometric changes over the land would be limited to evaporating and condensing the same water over and over again; and by absorption into the earth or by running into the sea even this water would soon go out of reach and precipitation would cease altogether. The winds are the vehicles which bring moisture, and as fast as one body of air is depleted another body of it takes its place. The quantity of air, then, which is to yield moisture is simply proportional to the velocity of the wind.

There is no doubt that the faster saturated air is supplied to a locality where it is cooled the faster (*ceteris paribus*) the rain or snow will fall. But the ratio of increment of wind velocity to increment of precipitation is not a simple one. It will be given farther on. Just here we are merely concerned with the inquiry, How would this air-supply or wind-velocity be affected by a warmer climate? Would it be increased? and, if so, in what ratio? Undoubtedly it would be increased. As regards the ratio, an answer will be attempted presently. The velocity of the wind is intimately associated with the second factor, rate of cooling, and it is first necessary to have the entire range of facts before us so as to dispose of the matter in its entirety.

(8.) Let us, then, consider the second factor which affects the rate of precipitation, viz: the *rate at which air cools;* and let us afterwards inquire how it would be affected by a change of climate consequent upon increased solar radiation. There are four known ways in which the cooling of air occurs: (1) by the work done in expansion; (2) by contact with colder surfaces; (3) by commingling with colder air bodies; (4) by excess of radiation over absorption of heat.

(9.) A moment's reflection will convince us that the first three modes of cooling are dependent altogether upon movements of air. In order that air may expand it must move upward; so, also, in order that it may be brought into constantly renewed contact with colder surfaces or may continuously intermingle with colder air bodies, it must have movement, and, therefore, velocity. The rates of expansion, contact and commingling are plainly proportional in a simple ratio to the velocities of the respective movements which cause them. Again comes up the subject of the cause, quantity and rates of these movements and here is the place to inquire how such movements would be affected by a warmer climate. This subject can be reached only by going back to the causes.

(10.) In an ultimate analysis the cause of air-movements is found in the incessant and unequal disturbances of the thermal equilibrium of the atmosphere. The sun's radiant energy falls unequally at any instant upon different places. It falls unequally at morning, noon and night; unequally upon different zones of the earth. It falls upon surfaces in the same zone which are heterogeneous with respect to their coëfficients of absorption; as, for example, land and sea. It penetrates an atmosphere which has unequal absorptive capacity in its different strata. The result is a disturbance of the temperature-equilibrium of the atmosphere in numberless places at once and incessantly. Consequent upon this is a disturbance of the statical equilibrium of the air; and further consequent is the rush of air to find a new equilibrium. Any disturbance of a

body from a position of stable equilibrium which it seeks to reëstablish, is a case of the conversion of kinetic into potential energy. The radiant energy which falls upon the earth is partially converted into potential which is in turn expended in producing the movements of the air. Having referred these movements back to their cause and having put the conditions of the question into the most general and comprehensive form, we may now ask again, would the wind-potential be greater if solar radiation were to increase? Undoubtedly it would. But in what ratio? To this latter question, I believe, a sufficiently approximate answer can be given. The problem is purely a thermodynamic one.

(11.) The atmosphere, considered with reference to its winds, may be regarded as a series of thermodynamic engines operated by an expansible and nearly perfect gas receiving heat and converting it into work. The amount of energy available for this work is directly proportional to the difference (in any given case) between the *absolute* temperature which the body of air performing work possesses, and the absolute temperature which is necessary for its statical equilibrium. We have seen that the causes which determine these differences of temperature—the sphericity of the earth, its rotation and seasons, its heterogeneous surface—are in the main fixed in nature and constants; while the motive power is the sun's radiant energy. And since the solar radiation and the temperature-differences of the air are both heat-quantities, pure and simple, we have, apparently, no alternative but to conclude that they are proportional to each other. But the exact form of the ratio is unknown. Nevertheless, if we assume it to be a simple ratio for any range of variation in the amount of solar radiation which could be reasonably postulated in connection with the present discussions, we shall certainly commit no large error. Still less shall we err if we assume that the inequalities in the heating of the air are proportional in a simple ratio to the mean absolute temperature of the air at the earth's surface, and if that temperature were raised by increased solar radiation the inequalities which cause the winds would increase in the same ratio.

(12.) How great an increase in the mean temperature of the earth's climates would the advocates of a warm glacial climate be disposed to postulate? Would 20° C. be sufficient? Taking the mean temperature of the earth's atmosphere at the surface to be (274°+16°) C. and adding 20° to it, we have 310°, and the ratio of $290 : 310 = 1{\cdot}07$, or an increment of seven per cent in the absolute temperature of the atmosphere. Assuming that the wind potentials are every where increased in like ratio, and remembering that the velocity of the wind is proportional to the square root of the energy expended in producing it, the

resulting mean velocity of the winds would be $\sqrt{1{\cdot}07}=1{\cdot}035$, or about three and one-half per cent greater than at present. But suppose the wind potentials increased in a geometrical ratio with the temperature of air. The result would of course depend altogether on the form of this ratio. But taking it in its simplest form (logarithm of the potential simply proportional to the absolute temperature) the increment in the wind-velocity, resulting from 20° increase of mean temperature, would be less than six per cent. Larger geometrical ratios can of course be arbitrarily postulated, but they would require a very stalwart defense to entitle them to a hearing.

(13.) Let us now go back and review our first two factors which determine the rate of precipitation; (1) air supply, (2) rate of cooling. We have seen that both are dependent—the first wholly and the second in great part—on the velocity of the winds. The first factor, air-supply, is evidently directly proportional to the velocity with which the winds move. But we have found reason to believe that this velocity would not be very much increased, though it would to some extent, by an increment in the mean temperature of the atmosphere which most thinkers would probably consider very large. The same conclusion attaches to the second factor, rate of cooling, in so far as it is dependent upon the velocity of the wind. But we have noted that this second factor depends for its value upon four subordinate or component factors, commingling, expansion, contact, and excess of radiation over absorption of heat. The first three depend for their value upon the velocity of air movements solely. The fourth component (excess of radiation over absorption) presents other considerations.

(14.) In the long run, radiation and absorption of heat by the atmosphere are equal. For if one or the other predominated continuously the air would grow continuously warmer or colder. Practically during any short period of time, and in every locality one or the other does predominate; but the ratio of the two perpetually oscillates to and fro about an equality. Now if air were motionless for a long period of time this oscillation of temperature would soon cease to precipitate moisture upon the land though the vibration might still continue. But in reality fresh air laden with new supplies of moisture is constantly replacing the bodies of air which have been depleted. Again we find that the movement of air is a vital consideration. But the nature of the dependence of that portion of the cooling caused by excess of radiation upon the velocity of the wind is different from that of commingling expansion, etc. In the latter operations their efficiency is proportional in a simple ratio to the velocity. Not so the efficiency of radiation. The law in this case is a more complex one and the ratio has less

value than a simple ratio. Inasmuch as it can be made intelligible only by the use of an algebraic expression, the analysis of it is given in the appended note.* The general result of that analysis is that when air moves from a warmer to a colder place, where it is cooled by radiation, the amount of cooling (by radiation simply) over any given area, does not increase in the same ratio as the velocity of the wind, but in a ratio which itself diminishes rapidly as the velocity increases.

(15.) We have thus examined the essential features of the first two factors which determine the rate of precipitation with reference to the changes they would probably undergo if the earth's climates became warmer. These changes we find to be very small for any increase of warmth which would be postulated. So small are they that hereafter they will be considered as unimportant.

(16.) The third factor which determines the rate of precipitation depends for its value upon the temperature at which the cooling of saturated air begins. It is by far the most important factor of the three. Its value is directly proportional to the maximum density of water vapor considered as a function of temperature, and it is well known that this density increases with the temperature, in a very rapid ratio. A roughly approximate idea of it may be derived from the fact that at such temperatures as we are most concerned with, this density, or, what is equivalent, the so-called capacity of air for moisture, is about doubled for an increase of 10° C. in the temperature of

* Let us consider an area of unit width over which air is passing. Suppose that while it is passing, some cause, the nature of which need not be specified, determines an excess of radiation over absorption of heat. It must have some "potential," which is measured in this case by the excess of the temperature of the air over that temperature at which radiation and absorption would become equal. The law is that the rate of radiation at any instant is proportional to the instantaneous value of the potential. But as the air cools, this potential is constantly diminishing. Let then P be the initial value of the potential when the air first reaches the supposed area and let p be its value after any time t, during its passage over it. Then the change dp in the value of the potential during any time dt (taken so small that during its duration p may be regarded as sensibly constant), is expressed by the Eq. $-dp=pdt$. Integrating this Eq. between the values P and p for the potential and between the corresponding values of 0 and t for the time, we have $\log p-\log \mathrm{P}=\log \frac{p}{\mathrm{P}}=-t$, and $p=\mathrm{P}\varepsilon^{-t}$. Now the total cooling during the time t of any unit volume of the air is equal to its loss of potential; that is, $\mathrm{P}-p=\mathrm{P}(1-\varepsilon^{-t})$. If t be taken as the whole time of passing, and if v represent the velocity, then $t=\frac{1}{v}$. Substituting this expression for t, we shall have the amount of cooling of unit volume of air by radiation while passing over area of unit width with the velocity v. But the quantity of air which passes is directly proportional to the velocity, and multiplying the expression by v we obtain the Eq.: Total cooling over given area $=\mathrm{P}v(1-\varepsilon^{-\frac{1}{v}})$. The examination of this term shows the general result stated in the text. The potential P may also be a variable and have a slightly increased value under a warmer climate, but any such increment would of course be very small.

the atmosphere. So obvious are the considerations which arise from this fact that no further discussion of it is necessary.

(17.) We find then that a warmer climate would probably have the effect of increasing the rate of precipitation from causes which may be grouped into three categories. First, the quantity of air which supplies the moisture would move a little more rapidly to the places when it cools. Second, the rate of cooling which causes precipitation would be a little faster. Third, since the cooling would occur in a higher range of temperature, the amount of precipitation per unit volume of air per unit degree of cooling would be rapidly increased. The last factor is very much more important than the other two. Hence the rate of precipitation would be largely increased by an increment in the mean temperature of the earth's climates. The general conclusion indeed is much too obvious to be disputed. But it has been deemed essential to go over the ground upon which it rests and analyze the component causes because they will come into service in the subsequent discussion.

(18.) Having found that the mean *rate* of precipitation would be largely increased by a warmer climate, the next step is to inquire whether the *time* of precipitation would also be increased by the same cause. Here as before we must recur to the causes and conditions, which fix for any locality, the number of rainy days and hours of the average year; but we only need to advert to them in their most general forms. We may recall again the statement that precipitation takes place when saturated air is cooled—never otherwise. It cools when it moves from a place where the local conditions make it warmer, to a place where the local conditions make it cooler. Again we find that the movement of air becomes an inseparable consideration. And in truth, we know by the commonest experience, that in almost all regions except very arid ones, the winds from certain directions bring wet weather, while those from other directions bring dry weather. The logic of it is, that the former winds come from warmer places laden with moisture and are cooled, while the latter come from places where they were colder, and become warm and therefore dry. The causes then which control *the directions* of the winds are the determinants of how much of the year shall be foul weather and how much fair. It may also be suggested that the relative humidity of the air is another determinant. But upon inspection it will appear that this possible factor is partially implied in the other, and we must not try to make our factors do double duty. In part, however, it presents independent considerations which will be adverted to presently.

(19.) The question then is how would a warmer climate affect the directions of the winds in any locality, and their relative humidity? There are some winds like the monsoons, the trades and anti-trades which appear to be governed by general laws, which (for better or for worse) have been formulated by theorists. But it seems quite certain that the trades and anti-trades would blow as unceasingly, and in the same directions under a warmer climate as under the present one; that the monsoon winds would begin and end at the same times of the year as they do now; and that this is true of all winds which have the seasonal or monsoon character. And they would presumably bring the same quota of rainy and dry days—neither more nor less. There are also what may be termed irregular winds of which the cyclone class are examples, but for the caprices and anomalies of which no law has been found. In high latitudes the winds are vacillating and their vagaries are still more obscure. Have we any reason to suppose that these winds as yet anomalous would yield any more wet days if the climate were warmer? I see none. Their alternations might be a little more rapid, and there might be a greater number of them in the course of the year, but the same reasoning would lead to the inference that the average duration of each oscillation would be diminished in exactly the same ratio that their number was increased, and so the total time would be unchanged.

(20.) As regards the humidity of the air it would seem as if the advocates of a warm glacial climate had confounded absolute humidity with relative. If it could be made to appear that the relative humidity would be greater under a warmer climate, the proof of it would have great weight. For no doubt cooling air often fails to precipitate because its relative humidity is low: and greater relative humidity would obviously cause a certain number of rainy or snowy days, where now there are merely cloudy ones. But there appears to be no reason for supposing it would be any greater. Indeed, is there any greater relative humidity to-day in tropical regions than in extra tropical, in the air as it leaves the ocean, or in air which has passed through equivalent changes and travel over the land?

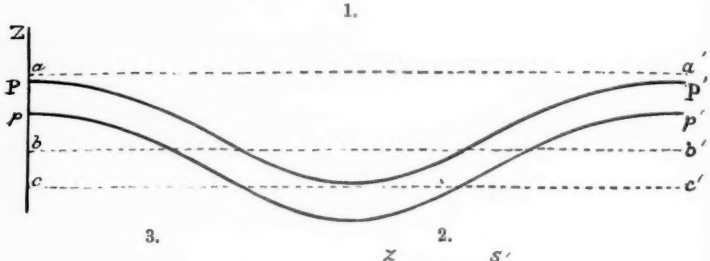
(21.) We do not find then any reason to suppose that either the directions or relative humidity of the winds has any relation whatever to the intensity or degree of solar radiation; but that so far as known laws can guide us they depend upon conditions which are exclusively terrestrial and therefore constant for all values of solar radiation. And as I am unable to conceive any possible cause of change in the time of precipitation which is not directly referrable to these two determinants I see no reason *founded on known causes* to suppose that a warmer climate

would affect the time of precipitation in any manner whatever. Possibly other inquirers may be more ingenious and more fortunate. Possibly the unknown operations of the intricate machinery of the winds might be made to yield another result to more subtle analysis. And indeed I am tempted to suggest here a line of thought which if carefully pursued might lead to some modification of the above provisional conclusion; though having had no opportunity to give it proper consideration I am unable to venture any opinion as to what the character of that result might be. Let us take an instance.

(22.) The western part of North America, from Southern Alaska as far south as the head of the Gulf of California, shows climates which increase in moistness as the latitude increases. Leaving out of view the climatal conditions prevailing high up on the mountains; the valleys to the east of the great barrier ranges, Sierra Nevada, Cascades, and Rockies of British Columbia, grow dryer toward the south. The rainfall—the number of rainy days or time of precipitation decreases with the latitude. The explanation of it I have suggested in a former paper of this Journal about as follows. In the northern portions of the region above specified the adjoining tracts of the Pacific Ocean are relatively warmer than the land; in the southern portions they are relatively colder. The prevailing winds are from the west. In the northern parts they are, during the greater part of the year, cooled as they pass into the interior of the continent, while in the southern parts they are heated. Hence at the north the same wind from the same ocean is a wet wind while in the south it is dry. Now it is quite conceivable (though from want of full consideration I should not dare to offer it as a probable result) that if solar radiation were to vary, the temperature-relations as between land and sea would also be profoundly changed. It is conceivable if the solar radiation were greater that the land at the north might heat the Pacific winds instead of cooling them. It is conceivable that they might cool them instead of heating. Both the winds and the lands would be warmer, but which of the two would have received the larger increment of temperature is the indeterminate question.

(23.) But for the present the only logical course which is open to us is to confine ourselves to plain and obvious results of physical laws. It is idle to argue about things which might be, or about things which in the present state of human knowledge are indeterminate. We have no apparent resource but to assume that a warmer climate would have no effect whatever in the way of increasing or diminishing the number of wet days and hours of an average year, and this is the assumption here made. The burden of proof is upon those who would argue otherwise.

(24.) We may now proceed to separate the snowfall from the rainfall. In a preceding paragraph attention was invited to the very obvious fact that moisture cannot be condensed in the form of snow at temperatures above zero. This fact with equally obvious ones connected with the changes of seasons are sufficient for whatever separation is required. In fig. 1, let the



axis of Z be the scale of temperatures, and let PP′ represent the temperature of precipitation throughout an average year—the higher parts of the curve corresponding to summer and the lower parts to winter months. Draw *pp′* below PP′ at such a distance as will adequately represent in all parts of the year the temperature to which the air falls in cooling. The two curves need not be exactly alike nor nearly so symmetrical as they are drawn. The forms here given merely express their general nature. The space between the two curves expresses the integral amount of cooling which the air undergoes in order to precipitate. If now the point *b* on axis Z be selected as the position of zero temperature, then so much of the space between the curves as lies below *bb′* represents the integral amount of cooling available for snow. If the climate to which the fore-

going is applicable be rendered warmer then the position of zero must be taken at a lower point on Z, as at *c*, or what amounts to the same thing the curves must be drawn higher up relative to the zero line. Then the integral amount of cooling available for snow will be represented by the space below *cc'* and within the two curves. It is evident that according to this construction the warmer climate would decrease the amount of cooling available for snow except in the case of one so cold that the temperature is always below zero—which may be represented by drawing the zero line above both curves at *aa'*.

(25.) But the same amount of cooling produces different amounts of precipitation in different parts of the temperature scale. While the amount of cooling available for snow has been diminished the efficiency of what remains has increased. In fig. 2, let Z, as before, be the axis for temperatures, and let *Ss* be the curve of saturation or maximum vapor density of water considered as a function of temperature. Let the vertical dimensions of the figures *f* and *g* represent equal amounts of cooling. Then their areas will exemplify the different amounts of precipitation in different parts of the temperature scale produced by equal amounts of cooling. To combine the two factors three axes are necessary.

(26.) In fig. 3, draw the three rectangular coördinate axes, OX, OY, OZ. Upon the plane YZ draw a convenient portion of the saturation curve *Ss*. Conceive the plane figure *Sszz'* to be a generatrix moving along the axis OX in positions always parallel to ZY. The line *zz'* will generate a vertical plane face, the line *zs* a horizontal plane face, and the line *Ss* a curved face of an indefinite solid. Conceive now two corrugated cutting edges similar to that of a sheet of corrugated iron and having the curvature generally expressed in fig. 1, by the curves PP' and *pp'* be passed into the indefinite solid, moving parallel to YZ, so as to cut out of it the shaded solid as drawn. This definite solid represents graphically the annual precipitation regarded as a function of the temperature and seasonal changes. The axis Z represents temperature; and let the zero point be arbitrarily fixed at the origin O. To represent the effect of an increased general temperature it would not be proper in this case to locate the zero point lower down on Z, but the solid must be cut anew from the indefinite solid by passing into it the corrugated edges higher up. The amount of snowfall is represented by that portion which lies below the plane XY.

(27.) In this graphic representation the annual precipitation is made to depend upon three functions between two variables. The two variables are temperature and time. The argument is that precipitation takes place only when saturated air is cooled. The quantity of precipitate which cooling air will yield (per

unit volume of air) is dependent upon the extent or amount of cooling, i. e., the difference between its initial and final temperature, and upon the temperature at which the cooling begins. This is expressed by the length and positions of the vertical lines in the front face of the solid. The curved line PP′ represents the locus of these initial temperatures for every day in the year, and the line *pp′* represents similarly the locus of the final temperatures. I have drawn them about equally apart for the whole year. As a matter of fact they should be unequal, most probably; for the range of cooling is not ordinarily uniform in the storms of different parts of the year. But it will appear farther on that this is of no consequence so far as the final conclusion is concerned. The reader may imagine the intervals between PP′ and *pp′* to be arranged in any way he likes provided it conforms better in his judgment to the facts in any case. The final conclusion will cover every admissible modification. Perhaps it will be said that at any given *phase* of the year when two consecutive years are compared there will be no definite initial and final temperatures and the amount of cooling even on corresponding dates will be very different. Quite true; but the illustration contemplates the averages of a very long term of years—hundreds of years or even thousands—and these averages will have perfectly definite initial and final temperatures of cooling, and every day and hour of the year is thus supposed to have definite conditions suitable to rainfall—unless indeed we consider a locality where during some part of the year rain *never* falls. In that case the curves become discontinuous, as does the solid also. The z ordinates then are a simple function of the temperature.

(28.) The y ordinates represent the quantity of moisture which unit volume of air precipitates at any temperature. They are proportional to the maximum vapor density of water which may be expressed by the product of an arbitrary constant into a term involving no other variable than temperature. Hence the y ordinates are functions of temperature. They express the efficiency of the cooling at different temperatures and therefore the differential amount of precipitation which any indefinitely small cooling will produce at any temperature. They may be combined with the z ordinates so as to yield a single function expressing the instantaneous rate of precipitation. The areas of the figures f and g, fig. 2, or any other cross-section of the solid by a plane parallel to YZ, will express the rate of precipitation regarded as a function of temperature. The x ordinates represent time which is used here as the independent variable. The temperature itself, and therefore the y and z ordinates, are harmonic functions of the time, the form of which is not exactly known.

(29.) The first effect to be considered is the shortening of the time of snowfall, so far as this time depends upon the changes of seasons. It is plain that the increment of temperature due to increased radiation must pervade every portion of the earth and throughout the entire year. Quite likely the increments would be unequal in different latitudes and unequal at different seasons. Still there would be an increase at all places and at all seasons. The summer would come earlier and stay longer; that is to say, the time during which it would be cold enough to snow would begin later in the autumn and end earlier in the spring.

(30.) The second obvious effect is that the rate of precipitation, whether for rain or snow, would be increased. For by the hypothesis the precipitation would be the result of cooling air at a higher temperature than before and equal amounts of cooling cause a larger amount of precipitation, the higher the temperature at which the cooling begins. In some slight degree the supply of air yielding snow would probably be increased through a small increase in the velocity of the wind, but this increment is considered as trifling.

Thus the time of snowfall would be diminished, but the average rate of snowfall would be increased. The amount for the year is simply the product of the time multiplied by the average rate of snowfall. Since one of the factors would be decreased and the other increased by the supposed change, the question hangs upon the answer to the inquiry—which of the two factors increases or diminishes in the higher ratio with the temperature. To this final question the general answer is that *the time of snowfall would decrease in a faster ratio than the rate of snowfall would increase, and the total annual snowfall would be diminished by a warmer climate.* There is an exception or rather a class of exceptions which will presently be adverted to. The proof of the proposition is simple and conclusive.

Since the warm weather is extended further into the autumn and begins earlier in the spring the only effect of the warmer climate is to push the snowfall of October (wholly or in part) forward into November, and the snowfall of November forward into December; to push the snowfall of March (wholly or in part), back into February, and that of February back into January. The snowfall which originally belonged to December and January, has simply disappeared. Meantime the former heat of June comes now in May and the heat of July goes over into August.* Two new thermal months have made their

* It will of course be understood that I use the word "month" here to avoid a circumlocution. I use it as a general expression for a short period of time, having a variable relation to the phase of the year, and a definite relation to the distribution of temperature throughout the year. This period may be of any length, from one day to six months.

appearance, hotter than any ever known before, and add their potency to the annual liquefaction. A period of snowfall in midwinter has disappeared without compensation; a period of melting heat in midsummer has made its appearance as a clear gain to the total liquefaction. This is the net result of the warmer climate.

(31.) Let us examine this a little more in detail, taking a special case by way of illustration. Let us consider the climate of a region situated in rather high latitudes, say in the neighborhood of 50° to 55°, where the present mean temperature of precipitation touches zero on the 10th of October. For three or four weeks before and after that date the storms will sometimes yield rain, sometimes snow—the rain at first being more and then less and less frequent until nothing but snow falls. Similarly in the spring (April 15th?) there is a date at which the mean temperature of precipitation rises up to the zero line and passes above it with a period on either side in which snow and rain alternate—the snowfall gradually vanishing. Thus we have four seasons, one of summer rainfall, one of winter snowfall, and two seasons (autumnal and vernal), where the rains and snows are dove-tailed with each other. It is also necessary to remember that we are now considering an average year as before described, and we must stop a moment to consider the elements of which that year is made up. Every calendar date has a certain time, rate, temperature, and amount, of precipitation which is found by averaging the supposed observations of precipitation occurring on that date for hundreds of years. It is immaterial whether we consider any particular date as having a certain number of wet and another number of dry hours, occurring as one storm, one daily interval of clear weather, or as hundreds of short intervals of a minute or two duration alternating with as many short dry intervals.

Suppose now a warmer climate supervenes with a heat increment sufficient to postpone the time at which the mean temperature of precipitation touches zero until the 15th of November. In that case the 15th of November takes the precipitation which now pertains to October 15th, subject to a qualification which will be mentioned speedily. November 20th takes the precipitation of October 20th and so on. Thus the winter is driven forward in time. On the other hand the spring comes earlier; and, in inverse order, the winter is driven backward. The former may be called the procession of snowfall, the latter the recession of snowfall. In mid-winter the procession and recession meet and crowd out entirely a certain period of time in which snowfall formerly occurred. But the rate of precipitation during mid-winter has increased because the temperature is now higher at which precipitation takes place. But this

higher rate is merely the same rate which prevailed formerly at an earlier date. In the first half of the winter, the original rates and amounts of precipitation have only been postponed to later dates without change of value. In the last half of the winter the original rates and amounts of precipitation have merely been anticipated on earlier dates without change of value. The rates and amounts which have been anticipated and postponed have taken the place of rates and amounts which have disappeared entirely and without compensation.

(32.) Let us now look at some of the qualifications to the foregoing conclusion. It is implied in the argument that during our average year, the number of wet and dry hours, respectively, would be sensibly the same on each and every date. As a matter of fact this is not true. Certain seasons are dryer or wetter than others, i. e. have more or fewer rainy hours per day or per month. How would our conclusion be affected by introducing this consideration into the argument? That will depend somewhat on the nature of the distribution of wet and dry hours throughout the year. In the first place it may be remarked that this distribution is governed and regulated partly by local causes and partly by fixed astronomical causes or relations. Presumably the causes would be constant for all values of solar radiation. If wet weather is more frequent near the equinoxes than in mid-winter, the argument would become *a fortiori*. But if wet weather has its maximum in mid-winter, the argument would be weakened and in an extreme case might be so affected as to show a greater snowfall with a warmer climate.

(33.) There is also one general exception which is independent of the distribution of wet weather throughout the year, and in which a warmer climate would produce increased snowfall. If a region exists any where on earth, such that the mean temperature of precipitation all the year round is considerably below zero, then a warmer climate will—up to a certain limit—have the effect of increasing the rate of precipitation without affecting the time, and hence there will be an increase of snowfall. But the moment the temperature of precipitation passes above zero in any part of the year, then the shortening of the time of snowfall begins and proceeds at maximum rate of shortening for any further increase of temperature, and thereafter the conversion of snow into rain will subtract more snow than the increased temperature of precipitation will add.

(34.) The possibility of obtaining a greater snowfall by a warmer climate then is limited to such localities as are now extremely cold—to localities situated either very near the poles, or at altitudes far above the present line of perpetual snow. In all other places a warmer climate would add to the rainfall and

actually subtract from the snowfall, while increasing at the same time the annual liquefaction. The advocates of a warm, glacial climate have committed a most extraordinary oversight in failing to perceive that the moisture, which they would add to the atmosphere, can fall as rain only. Not until the air has discharged as rain all the moisture in excess of the quantity which saturates it at zero, can it begin to yield snow. This consideration alone ought to have deterred them from such a doctrine and its mere statement might seem sufficient to refute the idea. But it has been deemed proper to investigate the subject at some length, and to examine each component factor in its proper relations, in order to make ourselves sure that what seems to be a complete answer at the first glance, is still complete, however it may be tested in detail.

ART. II.—*On the application of Wright's Apparatus for distilling, to the filling of barometer tubes;* by FRANK WALDO, Computer O. C. S. O.

[Communicated by permission of the Chief Signal Officer.]

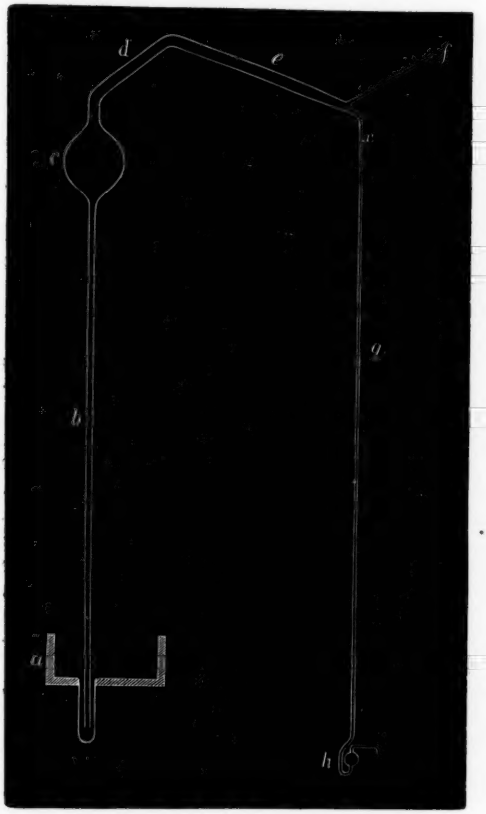
REFERRING to the original diagram by Professor Wright (page 480 this Journal, vol. xxii, December, 1881) and which is here reproduced by his permission, it will be seen that *a* is a vessel full of impure mercury, *b* a tube about thirty inches long; *c* an enlargement of *b*; *d* and *e* tubes inclined in opposite directions; *f* an arm for connecting with a Sprengel pump; *g* a tube a little over thirty inches long; *h* a reservoir with an outlet to the air; *h* is filled with pure mercury.

The air is now exhausted through *f*; the mercury rises in *b* and *g* until C is partially filled; a Bunsen burner is placed under *c* and the mercury distills over into *g* and flows out through *h*.

If now *g* is cut off at *x* a few inches below the junction of *e* with the arm *f* (the part *h* being no longer used) and a glass cock be inserted at *x*, then by means of a short rubber tube this cock can be connected with the open end of the barometer tube to be filled, which latter will take the general position of the whole tube *g* of Professor Wright's drawing.

The rubber tube must be covered with melted sealing wax. The impure mercury in *a* should first be washed in acids and dried before introduction. At the beginning of operations *a* is full of impure mercury, but the rest of the apparatus contains only air. The Sprengel pump is set in motion and gradually exhausts the air from *b*, *c*, *d*, *e* and the barometer tube, until no air bubbles can be seen in the running mercury of the Sprengel pump, and until the sharp click is heard when the drops of

mercury fall. The tube *f* is then sealed or a stop cock in it turned, cutting off the Sprengel pump; the Bunsen burner under *c* is lighted, and the mercury will distill over into the barometer tube, which will thus be filled without allowing the mercury to come into direct contact with the air.



The barometer tube should be constantly watched in order to detect any air bubbles that may be carried over; when seen they must be cooked out by heating the tube slightly by means of a Bunsen burner. When the barometer tube has become filled with the mercury, the cock at *x* can be closed, the sealing wax broken and the tube replaced by another.

This method is similar to the one employed by Wild at St. Petersburg, only he uses the Weinhold apparatus.

Hamburg, June, 1883.

ART. III. — *A New Device for Measuring Power;* by C. F. BRACKETT, Physical Laboratory of the College of New Jersey.

THE following account of a method of measuring the energy expended on or rendered by a dynamo- or a magneto-machine will be of interest to those who have to do with the production of electricity in the large way in which it is now employed in the enterprises of the day.

The machine is so supported, on uprights, that it can freely turn through a small arc of a circle whose center lies in the geometrical axis of the armature. The support may be effected by means of knife edges or by means of smooth cylindrical bearings, attached directly to the machine or to a cradle on which the machine rests. In the latter case the cradle is made adjustable so that the bottom or floor can be raised or lowered, thus permitting machines of different construction, when placed thereon, to be brought into proper positions as regards axis of revolution and points of support. When the machine, thus mounted, is set in rotation, with closed circuit, the mechanical couple set up between the armature and field magnets tends to make the latter revolve in the same direction with the armature. The value of the couple, thus operative, and which we desire to know, will be known if we know the value of the couple, equal and opposite in direction, which is required to hold the machine fixed in its position of equilibrium. A lever arm is fixed to the machine or cradle in a horizontal position and provided with a sliding weight of known value, sufficient to hold the machine fixed in its position of equilibrium when in the performance of its duty. The couple required can thus be known in terms of lever arm and weight. We then only need to know the number of revolutions in a unit of time when we have all the data needed in order to compute the energy.

If W denote the weight, L the lever arm and n the number of revolutions in a minute, we shall have: energy $= 2\pi W L n$, as in the case of the well-known Prony brake.

For purposes of accurate scientific inquiry, the field magnets alone may be mounted and balanced on knife edges, so as to turn freely like the beam of a common balance. By this plan all useless work is excluded from the account. Friction at the bearings and at the brushes do not in this case have any tendency to make the field magnets revolve.

In the Physical Laboratory of this institution there are several machines having the construction here pointed out. They leave nothing to be desired in point of sensitiveness or accuracy in their indications.

Princeton, Nov. 23, 1883.

ART. IV.—*On some points in Climatology. A rejoinder to Mr. Croll;* by SIMON NEWCOMB.

IN the number of this Journal for October, 1883, Mr. Croll publishes a reply to certain criticisms of mine urged seven years ago against his theory of the cause of glacial epochs. (Vol. xi, p. 263, May, 1876). The pleasure and interest with which I have read Mr. Croll's paper induces me to reply to it, notwithstanding a want of confidence on my part in the value of anything short of a purely mathematical investigation of the subject. It will be well to begin by examining the nature of the question and stating in a broad way what seems to me unsatisfactory in the foundation of Mr. Croll's method.

What we are concerned with is the inference that at some former epoch in geological history the mean temperature of the northern hemisphere was much lower than it is now. Assuming this as the basis of discussion, the question is, what was the cause of this "glacial epoch?" To speak more accurately; since we can only take the causes relatively, why was the northern hemisphere any colder then than it is now? This question Mr. Croll endeavors to answer from purely astronomical causes, combined with elementary considerations respecting the motion of heat and its relation to meteorological phenomena. His conclusion is that a great eccentricity of the earth's orbit, combined with a position of the perihelion near the northern solstice, will cause a great annual fall of temperature in the northern hemisphere, which, in such a case, would have a short perihelion summer and a long aphelion winter.

To this, my reply is, that too little is known of the laws of terrestrial radiation of heat through the atmosphere to justify the establishment of any theory of the glacial epoch, and that, taking the case up exactly as Mr. Croll does, he fails to show sound reason why the mean temperature should be different at the supposed periods. At the same time, my verdict would be, not that Mr. Croll's thesis was false, but that it was not proven. I do not deny the possibility that, when the laws of climate become thoroughly known, it may be found that epochs of great eccentricity are always glacial epochs. All I claim is that if such should be proven to be the case, it will be through the action of causes different from those adduced by Mr. Croll.

In fact, without going any further, we have at hand a *vera causa* acting in this direction which has not been considered by Mr. Croll at all. Experiments on radiation, commenced with Dulong and Petit, tend to show that Newton's theory of the proportionality between temperature and radiation is not well

founded, and that, as temperature rises, radiation increases in a much higher ratio. To speak more exactly, if we take a series of temperatures in arithmetical progression, the corresponding rates of radiation of heat will not be in arithmetical progression, but in a series of which the differences continually increase. An immediate inference from this general law is that if an isolated body receive a given amount of radiant energy per annum, its mean annual temperature will be a maximum when this radiation is uniform and will be lower the more irregular the reception of heat.

Now it is well known that the total amount of heat received, not only by the earth as a whole, but by each hemisphere, is constant, notwithstanding the change in the earth's eccentricity, but in virtue of the law just stated, any portion of the earth's surface on which a large portion of the annual supply of heat is delivered during a short summer, will have a lower mean temperature than the hemisphere on which the heat is distributed more uniformly. But Mr. Croll does not, so far as I have ever noticed, adduce this law at all. On the contrary, he assumes Newton's law of radiation proportional to temperature under which the cause would not act in the way suggested.

One great source of in-conclusiveness in Mr. Croll's results seems to me to be a lack of quantitative precision in his language. Though he may use numbers wherever it seems to him they are applicable, one can hardly fail to notice that the quantitative terms he most uses are such as "great," "very great," "small," "comparatively small," and these without any statement of the units of comparison relatively to which the expressions are used. Now I deem it not improbable that the difference between a cold and a hot epoch may be due to the very small preponderance of one or the other of several antagonistic causes; and, if so, quantitative precision is necessary to lead to any reliable conclusion.

I shall now enter into some details: Mr. Croll suggests that I may have forgotten the researches of Pouillet and Herschel into the temperature of space. I reply that I regard the conclusion that the temperature of space is $-239°$ as having no sound basis. To speak with greater quantitative exactness, it has precisely the same value as a photometric estimate of the intensity of star light, founded on observations of the sky, made in full day, with an attempt to eliminate the light reflected by the sky so as to find what residue comes from the stars. The fact is, that no observations of radiant heat from stellar spaces at large can be made below the uppermost limits of the earth's atmosphere, owing to the intervention in lower regions of the radiation from the atmosphere itself.

Mr. Croll concludes, using Newton's law of radiation, that the heat received from the stars is to that received from the

sun as 222 to 299. I wonder that he did not see in this a *reductio ad absurdum* either of the results of Pouillet and Herschel, or the law of radiation which he assumes. Photometry shows that the combined light from all the stars visible in the most powerful telescope is not a millionth of that received from the sun, and there is no reason for believing that the ratio of light to heat is incomparably different in the two cases.

In considering the question of the heat conveyed by aerial currents Mr. Croll quotes from my former paper so fully and fairly that I do not see any necessity to repeat my views at length. I can only say that while I now see more plainly than before some reason why a body at the upper region of the earth's atmosphere should, on the average, be colder than at the surface, I do not see that we have data for fixing the fall of temperature at 5° or 100°. If the degree of cold is greater than that due to expansion, then Mr. Croll is right in maintaining that the aerial current would not carry to the poles *all* the heat with which it left the equator, but even granting this condition I see no ground for supposing the quantity of heat conveyed to be insignificant.

I shall now consider some of Mr. Croll's reasons why the ocean should be warmer than the land. His assumed law that a body transparent for heat rays would become warmer under solar radiation than an opaque body, I passed over in my former criticism as too much opposed to the fundamental laws of thermodynamics to need much consideration. He now adduces, in support of his thesis, the fact that water is more transparent to the solar rays than the rays which it would itself radiate; and that the upper layers of water would act like the glass of a green house and thus allow the water to stand at a higher temperature than it would otherwise do. This addition to the *modus operandi* seems to me quite sound, and, therefore, to show one true cause why water might rise to a higher mean temperature than the land, though I am unable to say whether the increase would be measurable with an ordinary thermometer. But I am sorry to find that, notwithstanding his addition of a sound cause, he adheres to views so diametrically opposite to what I supposed to be the fundamental laws of thermodynamics that I feel compelled to state the case more fully. His first reason why the ocean should be warmer than the land, is in the following words:

First.—"The ground stores up heat only by the slow process of conduction, whereas water, by the mobility of its particles and its transparency for heat-rays, especially those from the sun, becomes heated to a considerable depth rapidly. The quantity of heat stored up in the ground is thus comparatively small, while the quantity stored up in the ocean is great."

As just remarked Mr. Croll substitutes a sound reason for this utterly bad one, but still seems inclined to hold on to the latter. The confusion of ideas which pervades it can best be shown by making some attempt to put the statements into quantitative language, using numbers, lengths, etc., instead of the qualifying words "slow," "considerable," "rapidly," "comparatively small" and "great." His statement would then read something in this shape: The ground stores up heat only by the process of conduction which admits of only 10 calories per square meter being absorbed in a day whereas water, by the mobility of its particles, etc., becomes heated to a depth of thirty feet at the rate of 1° Fahr. per hour (day or week as the case might be). Thus only 1000 units of heat are stored up in a cubic meter of earth, while 5000 units per cubic meter are stored up in the ocean.

When stated in this form the question how hot the ocean would get at the end of x days, weeks or years under the supposed law of heating and how the number of units of heat stored up respectively in the ground and the ocean would fix their respective temperatures would at once have arisen in Mr. Croll's own mind, and showed him the utter failure of his reasoning; but by using instead of numbers the qualifying phrases I have quoted he confuses integral quantity of heat, rate at which heat is radiated in a unit of time, heat stored up, and temperature, without destroying the apparent soundness of his argument in the mind of the uncritical reader.

The second reason is in the following words:

Second.—"The air is probably heated more rapidly by contact with the ground than with the ocean; but, on the other hand, it is heated far more rapidly by radiation from the ocean than from the land. The aqueous vapor of the air is to a great extent diathermanous to radiation from the ground, while it absorbs the rays from water and thus becomes heated."

Here again the fallacy of the reasoning will be seen by giving the respective number of degrees, or any quantitative statement of the rate at which the air was heated by radiation from the ocean and from the land respectively. The fact I suppose to be that there is no rapidity of heating in question, but that the question is simply one of stationary temperature to be ultimately reached. I must repeat that I know not the slightest authority for the statement in the last sentence quoted and can gain no clear idea from what Mr. Croll says on the subject.

In considering the third reason, which I need not quote, but which is found in Mr. Croll's reply, I suggested in my former paper what I supposed to be a *reductio ad absurdum* of Mr. Croll's method of reasoning by pointing out the apparent conclu-

sion that two bodies could heat each other up by their mutual radiation. I supposed he would disclaim this conclusion and try to show that I had misunderstood his premises in drawing it, but he apparently accepts its possibility as a logical result of Prevost's well known theory of exchanges.

The fourth reason may be summarily disposed of in the same way as the preceding ones. Let the reader take it up as presented by the author; let him substitute quantitative statements at pleasure for the words "more freely," "greater," "greater difficulty," "more rapidly," "of higher mean temperature," etc., and let him also bear in mind that it is stationary temperatures and not quantities of heat with which we are ultimately concerned and the inconclusive character of the reasoning will be at once apparent.

I shall next pass to the question of the non-melting of snow during a short perihelion summer, in which, as I stated in my former review, calculating temperatures by Mr. Croll's formula, we should have a mean temperature ranging from 100° to 150° Fahr. I had to acknowledge some embarrassment from Mr. Croll's causes producing their effects through the two diametrically opposite modes of operation, to wit:

1st. By making the air exceedingly transparent and thus permitting radiation into space.

2d. By filling the air with fogs and thus preventing the solar heat from reaching the ground.

His reply to this is that he did not suppose the fogs and the clear atmosphere to exist at the same place and at the same time, but that in either case an inability on the part of the sun's rays to melt the few inches of snow which could have fallen during winter would have resulted.

I see no use in arguing this point for the simple reason that I do not know enough about the relations of temperature to the aqueous vapor in the atmosphere to admit of my saying anything of value on the subject. I would merely remark that I cannot see in Mr. Croll's reasoning the slightest ground for admitting that the perihelion summer radiation would produce any other effect than it does now.

I am surprised that Mr. Croll should have been willing to present reasoning so obviously inconclusive as that in which he endeavors to show that my objection to the reliableness of his dates for glacial epochs, on account of the insufficiency of the fundamental data for the secular variations of the planetary orbits, falls to the ground. My objection and his statement in reply I can leave to the judgment of the reader who chooses to refer to them.

I conceive that some general remarks on the nature of the problem will be of more value than a further analysis of Mr. Croll's reasoning. It is an observed fact that we now have a

glacial epoch at a comparatively moderate height in the atmosphere and on the tops of most high ranges of mountains far removed from the equator. It is evident that if, at any former epoch, the state of things at the surface of the ground was the same that it now is at the height of two or three miles in the atmosphere, there must have been a glacial epoch. To what cause are we to attribute the cold of the upper regions of the air? There are two known causes but we cannot assign an exact quantitative effect to each.

I. The passage of air from the lower to the upper regions is accompanied by expansion, and the reverse motion by compression, which would naturally result in the upper regions being colder than the lower: the exact amount of cooling, supposing no disturbing cause to come into play, is readily computed, and has, I think, been assigned by Professor Sir William Thompson and others, but I need not now refer to the results.

II. Researches on radiant heat seem to show that the atmosphere absorbs the extreme rays of the spectrum, especially those of greatest wave length, more powerfully than the rays of mean wave length. The rays radiated by the earth are of longer wave length than the great mass of those received by the sun. The natural result of this selective absorption would be to make the temperature of the earth higher than if there were no atmosphere, or if the atmosphere exercised no selective absorption on heat rays. It seems probable that this selective absorption is due, very largely if not entirely, to aqueous vapor in the air. If this be so, an epoch of dry air would be a glacial one.

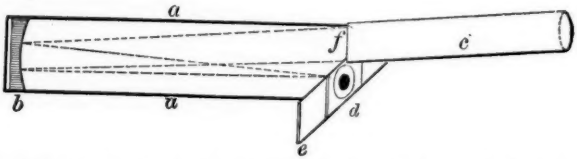
A crude test of the efficacy of the first cause might be devised. In order that it may act it is essential that there shall be a continuous interchange of air between low and high altitudes. Now if there are any high table lands so extended that in their central portions the air has not during several days an opportunity to be replenished from lower regions, such air should be warmer than that at an equal height on isolated mountains. Probably the conditions for such an observation do not exist on the earth's surface.

In conclusion I may be allowed to express my regret at not being able to make a contribution of positive value to the investigation of this subject. The state of the question is about this: A well founded theory of terrestrial temperature can be built only upon an accurate knowledge of the laws of emission and absorption of radiant energy of different wave lengths, especially in the atmosphere, and the result will appear as a numerical calculation, more or less exact, of the temperature resulting from assigned conditions, and not as the conclusion of an argument to show one thing or another.

ART. V.—*On Photographing the Solar Corona without an Eclipse;* by WILLIAM HUGGINS, D.C.L., LL.D., F.R.S.

[IN the number of this Journal for last February is a statement, by Dr. Huggins, of his method of photographing the corona of the sun without an eclipse. The following account of his more recently devised methods, and of the results obtained, was communicated to the British Association at Southport, and is furnished by him in proof from the British Journal of Photography for this Journal.]

I am indebted to Miss Lassell for the loan of a seven-foot Newtonian telescope made by the late Mr. Lassell. The speculum, which is seven and a-quarter inches in diameter, possesses great perfection of figure, and still retains its original fine polish. I decided not to use more than three and a-half inches of the central portion of the speculum—partly for the reason that a larger amount of light would be difficult of management, and partly because this restriction of the aperture would enable me to adopt the arrangement which is shown in the diagram.



It will be seen at once from an inspection of the diagram that in this arrangement the disadvantage of a second reflection by the small mirror is avoided, as is also the mechanical inconvenience of tilting the speculum within the tube, as in the ordinary form of the Herschelian telescope. The speculum *b* remains in its place at the end of the tube *aa*. The small plane speculum and the arm carrying it were removed. The open end of the tube is fitted with a mahogany cover. In this cover at one side is a circular hole *f*, three and a-quarter inches in diameter, for the light to enter; below is a similar hole, over which is fitted a framework to receive the "backs" containing the photographic plates, and also a frame with fine ground glass for putting the apparatus into position. Immediately below, towards the speculum, is fixed a shutter with an opening of adjustable width, and which can be made to pass across more or less rapidly by the use of india-rubber bands of different degrees of strength. In front of the opening *f* is fixed a tube *c*, six feet long, fitted with diaphragms, to restrict as far

as possible the light which enters the telescope to that which comes from the sun and the sky immediately around him. The telescope tube *aa* is also fitted with diaphragms, which are not shown in the diagram, to keep from the plate all light except that coming directly from the speculum. It is obvious that when the sun's light entering the tube *f* falls upon the central part of the speculum the image of the sun will be formed in the middle of the second opening at *d*, about two inches from the position it would take if the tube were directed axially to the sun. The exquisite definition of the photographic images of the sun shows, as was to be expected, that the small deviation from the axial direction—two inches in seven feet—does not affect sensibly the performance of the mirror. The whole apparatus is firmly strapped on to the reflector of the equatorial, and carried with it by the clock motion.

The performance of the apparatus is very satisfactory. The photographs show the sun's image sharply defined; even small spots are seen. When the sky is free from clouds, but presenting a whity appearance from the large amount of scattered light, the sun's image is well defined upon an uniform background of illuminated sky, without any great increase of illumination immediately about it. It is only when the sky becomes clear and blue in color that coronal appearances present themselves with more or less distinctness.

In my earlier work with this apparatus I used cells containing potassic permanganate in solution, which were placed close to the sensitive surface, and between it and the shutter. I was much troubled by the rapid decomposition of the potassic permanganate under the influence of the sun's light. When apparently clear to the eye, a lens revealed minute particles which precipitated themselves upon the glass plates of the cell, and gave an appearance of structure to any coronal appearance which was in the plate; besides, any diminution of the transparency of the solution by the presence of minute particles would produce scattered light on the plate.

I then tried a solution of iodine in carbon disulphide, but the same inconvenience presented itself. Very soon, under the sun's light, the solution was found by examination with a lens to show signs of commencing decomposition.

Even when the solution was sensibly clear there was some disadvantage from the unavoidable imperfection of polish of the surface of the plates, which reveals itself under the conditions of strong light in which they are placed. If, however, the violet (pot) glass which I used at first could be obtained annealed and free from the imperfections usually present in it, it would serve most usefully as a selective screen.

For these reasons, after some months' work I decided to give

up the use of absorbing media, and I came to the conclusion that the advantages they present, which are doubtless considerable, are more than balanced by the possible false appearances which they might give rise to if the solutions were not in a condition of perfect transparency.

As, for the reasons stated above, it seemed desirable to avoid placing media of any kind before the sensitive surface, the selective power upon the light had to be sought in the nature of the sensitive surface itself. The suggestion of staining the film presented itself, but after consultation with Captain Abney I decided to try an emulsion containing silver chloride only. Captain Abney kindly prepared some silver chloride emulsion for me, and the plates were developed with a solution of ferrous-citro-oxalate. The silver chloride film, according to Captain Abney, is strongly sensitive to light from *h* to H, and hardly at all beyond H. Since the middle of July these plates have been used as well as the ordinary silver bromide gelatine plates. A comparison of the two kinds of plates, when used under similar conditions, shows a decided advantage for this work in favor of the silver chloride. All the plates were backed with a solution of asphaltum in benzole.

For the purpose of screening the sensitive surface from the intensely bright image of the sun, small circular discs of thin brass were turned about $\frac{1}{50}$th of an inch larger in diameter than the sun's image. The brass disc was held close before the sensitive surface by a fine metal arm when the sun was taken in the middle of the field, and attached to the inner edge of a circular diaphragm when the sun's image was placed toward the side of the field. A comparison of photographs taken under similar conditions with and without the disc showed less advantage in favor of the disc than was anticipated. Indeed, it may be that with the short exposures given the scattered light which comes upon the plate, when the sun's image falls directly on the sensitive surface, may be favorable to the setting up of the photographic action by the comparatively-feeble coronal light.

In consequence of the number of diaphragms which it was found desirable to introduce into the apparatus for the purpose of preventing any light but that from the sun and the sky immediately around him from reaching the plate, the extent of field in which the full aperture was in use was small. For this reason it was found of advantage to place the sun's image near the margin of the diaphragm, limiting the field, and afterwards to combine the photographs taken in four different positions.

The moving shutter being placed very near the sensitive surface, and practically in the focal plane, could not give rise to effects of diffraction upon the plate; besides, the opening in

the shutter was never less than half-an-inch in width, and often as much as an inch or even more, according to the sensitiveness of the plates used.

The most serious difficulty with which I have had to contend has been the absence of clear skies. On many days of bright sunshine the wind has been in a northerly direction, bringing here the smoke of London, which produces a whity condition of sky, through which it was obviously hopeless to expect the coronal light to show itself upon the plates. The few occasions of a better condition of sky were for the most part of short duration, and did not allow time for a large number of photographs to be taken.

During the summer about three dozen photographs have been obtained, which show photographic action about the sun of a more or less coronal character.

I placed these plates in the hands of Mr. Wesley, who has had very great experience in making drawings from the photographs taken during several solar eclipses, with the request that he would make a drawing for each day on which sufficient photographs had been taken, combining the results of the different photographs in one drawing. This was desirable, as, whenever a sufficient duration of sunshine permitted, photographs were taken on silver chloride films as well as on silver bromide plates. Some photographs were taken with the sun screened by the brass disc, others without it; also photographs were taken with the sun in different positions of the field. As a rule, Mr. Wesley has introduced into his drawings those coronal features only which are common to all the plates taken on that day.

The apparatus is attached to the refractor of the equatorial in such a way that the direction of the length of the plate is in that of a parallel of declination; a line, therefore, across the plate, is in a direction north and south, and from the date of the photograph the angle of position of the sun's axis can be found. On Mr. Wesley's drawings the orientation is marked, as well as the position of the sun's axis.

Four drawings accompany this paper. On one of them (August 13) are seen defined rays. As these are present in three photographs—one in which the sun is in the middle of the field and the shutter in use, a second in which the sun was nearly in the middle but the shutter remained open, and a third with the sun near the margin of the field and screened by a disc—Mr. Wesley has put them in the drawing. In most of the negatives more structure than is shown in the drawings is suspected when the plates are carefully examined.

I regretted greatly that on the sixth of May—the day of the solar eclipse—the sky here was very unfavorable.

Up to the time of writing this paper I have not seen the photographs taken during the eclipse. Mr. Wesley wishes me to say that he has not seen the photographs or any drawings of the eclipse, and that, therefore, he has been wholly without bias in making his drawings from my plates. If these drawings are compared with the photographs taken during the eclipse, it should be borne in mind that the absence of sky-illumination during the eclipse would allow a larger part of the fainter and more distant regions of the corona to be photographed, and that any peculiar conformations or detailed structure of these outer portions could not be expected to be seen on my plates. The comparison should be restricted to the regions of the corona at corresponding distances from the sun's limb. It is probable that the short-exposure eclipse negatives will be found to admit of comparison with my plates better than those exposed for a longer time.

Photographs of the sun have been taken on the days which follow:—

April	2	1 plate.	June	20	1 plate.
"	3	1 "	July	10	3 plates.
"	6	2 plates.	"	15	2 "
"	26	5 "	August	3	2 "
May	23	1 plate.	"	13	7 "
"	24	6 plates.	"	20	7 "
"	31	5 "	Sept.	4	4 "
June	6	3 "			

All these plates show a more or less distinct coronal appearance about the sun. On some of the days an unfavorable wind brought here the London smoke, which greatly increased the sky-illumination relatively to the coronal light which could reach the plate. On these days the photographic action on the plates around the sun, though distinctly coronal in character, possesses less definiteness of form. I entertain the hope that it may be possible, by a careful comparison of all the plates, to gain some information, in a general way, of the amount, and possibly also of the character, of any large changes of form or of relative brightness which may have taken place in the corona, or been due to its motion, during the period covered by the observations.

[Professor Stokes, who read the paper, also read the following letter from Mr. Lawrence, one of the observers of the eclipse of May 6th, at Caroline Island.]

"Dr. Huggins called upon Mr. Woods this morning and showed us the drawings Mr. Wesley has made of his coronas. He told us that he particularly did not wish to see our negatives,

but he would like us to compare his results with ours. We did so and found that some strongly marked details could be made out on his drawings, a rift near the north pole being especially noticeable. This was in a photograph taken on April 3d, in which details of the northern hemisphere are best shown; while the details of our southern hemisphere most resemble the photograph taken on June 6th. In fact our negatives seem to hold an intermediate position. Afterwards I went with Dr. Huggins and Mr. Woods to Burlington House to see the negatives. The outline and distribution of light in the inner corona of April 3d is very similar to that on our plate which had the shortest exposure, the outer corona is, I think, hidden by atmospheric glare. As a result of the comparison, I should say that Dr. Huggins' coronas are certainly genuine as far as 8′ from the limb."

Art. VI.—*Elliptic Elements of* Comet 1882, I; by F. J. Parsons.

The accompanying set of elements was derived from the following six normal places:

G. M. T.	Mean R. A. 1882.0.	Prob. Error.	No. of Obs.	Mean δ 1882.0.	Prob. Error.	No. of Obs.	Time of Normal places.
March 26.5	271 36 46·9	±0·71	63	37 15 15·6	±0·41	61	Mar. 19–Apr. 3
April 12.5	281 22 16·7	±0·20	70	49 49 15·1	±0·66	70	Apr. 4–Apr. 21
May 25.5	55 55 34·2	±0·45	40	59 36 4·1	±0·28	40	May 21–May 28
June 2.5	66 45 42·3	±0·56	18	44 36 27·1	±0·53	17	June 1–June 5
July 9.5	156 59 16·35	±0·59	27	9 45 17·5	±1·3	27	July 8–July 17
August 7.5	181 46 55·5	±1·17	12	4 14 3·7	±1·3	12	Aug. 1–Aug. 16

From the 2d, 3d and 5th of these normal places a preliminary set of elements was computed according to the methods given in Gauss' "Theoria Motus," as follows:

$$T = \text{June } 10{\cdot}52908 \qquad \Omega = 204^\circ\ 56'\ 18''{\cdot}61$$
$$\log g = 8{\cdot}7836381 \qquad \omega = 208^\circ\ 59'\ 38''{\cdot}17$$
$$\log e = 9{\cdot}9999998 \qquad i = 73^\circ\ 48'\ 34''{\cdot}88$$

Mean eq. 1882·0.

Rectangular equatorial Coordinates

$$x = r\,[9{\cdot}9611128]\ \sin\,(126^\circ\ 22'\ 53''{\cdot}24 + v)$$
$$y = r\,[9{\cdot}8608368]\ \sin\,(\ 61^\circ\ 11'\ 47''{\cdot}56 + v)$$
$$z = r\,[9{\cdot}9021136]\ \sin\,(196^\circ\ 51'\ 24''{\cdot}51 + v)$$

With these elements considering the eccentricity equal to unity, comparison was made with all six normal places, in order to make the final corrections to the preliminary orbit by means of a least square solution.

The differential coefficients with respect to the six elements being computed for the six normal places gave twelve equations involving the following unknown quantities,

$$x = 10000\ d\mathrm{T}.$$

$$\mathrm{M}\sin y = d\log q \text{ (Briggian) or } y = \frac{dq}{q\sin 1''}$$

$$z = d\chi' = d\omega' + \cos i'\, d\Omega' \qquad v = d\Omega'$$

$$\sin u = de \text{ or } de = \frac{u}{\sin 1''} \qquad w = di'.$$

It was not thought worth while to apply weights systematically according to the number of observations, but as the last normal place seemed to have considerably less precision than the others, the last two equations were given a weight of 0·5012 log 9·70000 as a convenient approximation to a weight of one-half.

The twelve resulting equations being solved according to the method of least squares give the following values for the unknown quantities:

$x = 4''\cdot4771 \pm 1''\cdot468$	$u = -\ 2''\cdot2662 \pm 0''\cdot877$
$y = \ 2\cdot4213 \pm\ 4\cdot214$	$v = +10\cdot952 \pm 2\cdot495$
$z = -1\cdot4295 \pm\ 0\cdot875$	$w = +\ 9\cdot035 \pm 2\cdot739$

Substituting these values in the equations of condition gives for the sum of the squares of the residuals $[vv] = 22\cdot02$. In the solution of the normal equations $[nn\cdot 6] = 22\cdot09$.

From these unknown quantities the following corrections to the preliminary elements are found

$d\mathrm{T} = +0\cdot000448$	$\pm 0\cdot000147$	$de = -0\cdot00001098$	$\pm 0\cdot00000425$
$d\log q = +0\cdot0000051$	$\pm 0\cdot0000089$	$d\Omega' = +10''\cdot952$	$\pm 2''\cdot495$
$d\omega' = -8''\cdot024$	$\pm 2''\cdot644$	$di' = +\ 9''\cdot035$	$\pm 2''\cdot739.$

These corrections give as the most probable values of the elliptic elements,

$$\mathrm{T} = \text{June } 10\cdot52953 \quad \text{G. M. T.} \pm 0\cdot00015$$

$$\log q = 8\cdot7836432 \pm 0\cdot0000089$$

$$e = 0\cdot99998902 \pm 0\cdot00000425$$

$$\left.\begin{aligned} \omega &= 208^\circ\ 59'\ 33''\cdot72 \\ \Omega &= 204^\circ\ 56'\ 29''\cdot49 \\ i &= \ 73^\circ\ 48'\ 41''\cdot82 \end{aligned}\right\} \text{Mean Equinox and Ecliptic } 1882\cdot0$$

$$\left.\begin{aligned} \omega' &= 196^\circ\ 51'\ 16''\cdot49 \pm 2''\cdot644 \\ \Omega' &= 210^\circ\ 29'\ 12''\cdot25 \pm 2''\cdot495 \\ i' &= \ 52^\circ\ 57'\ 41''\cdot04 \pm 2''\cdot739 \end{aligned}\right\} \text{Referred to Equator.}$$

Rectangular equatorial coördinates,

$$x = r\,[9\cdot9611023]\ \sin\,(126^\circ\ 22'\ 49''\cdot33 + v)$$
$$y = r\,[9\cdot8608362]\ \sin\,(\ 61^\circ\ 11'\ 58''\cdot04 + v)$$
$$z = r\,[9\cdot9021280]\ \sin\,(196^\circ\ 51'\ 16''\cdot49 + v)$$

These elements correspond to a period of about 400,000 years.

Recomputing the position of the comet for the dates of the six normal places and comparing gives the following results, the column headed "v" being the residuals obtained by substituting in the equations of condition the values found above for the unknown quantities.

		$\delta a \cos \delta$ (C−O)	v	$d\delta$ (C−O)	v
March	26.5	+1·36	+1·34	+1·01	+1·00
April	12.5	−0·86	−0·71	−1·31	−1·31
May	25.5	+1·53	+1·52	+0·19	+0·17
June	2.5	−0·61	−0·60	+0·04	+0·03
July	9.5	+0·37	+0·38	−2·33	−2·16
August	7.5	−0·91	−0·95	+4·30	+4·24

The planetary perturbations being small have not as yet been taken into account, as I intend to make a more complete discussion of the comet when all the observations have been published.

Field Memorial Observatory, Williamstown, Mass.

ART. VII. — *The Minnesota Valley in the Ice Age;** by WARREN UPHAM.

THIS article is based upon my observations as assistant on the Geological and Natural History survey of Minnesota, from 1879 to the present time, under the direction of Professor N. H. Winchell, the State geologist. Some portions of it have been before published in the annual reports, and others are here presented in advance from manuscripts prepared for the final reports of this survey.

The Minnesota River, from which this State is named, has its highest and most remote sources upon the Coteau des Prairies, about 2000 feet above the sea. Its head-stream, after flowing eastward twenty miles, turns southerly at Brown's Valley and enters the northwest end of Big Stone Lake. Here, and in its whole extent thence to its mouth, the Minnesota River occupies a very remarkable valley, the origin of which was first explained in 1868 by General G. K. Warren, who attributed it to the outflow from an ancient lake that filled the basin of the Red River and Lake Winnipeg. This valley or channel begins at the northern part of Lake Traverse and first extends southwest to the head of this lake, thence southeast to Mankato,

* Read August 16, 1883, at the Minneapolis meeting of the American Association for the Advancement of Science.

and next north and northeast to the Mississippi at Fort Snelling, its length being about two hundred and fifty miles. Its width varies from one to four miles, and its depth is from one hundred to two hundred and twenty-five feet. The country through which it lies, as far as Carver, about twenty-five miles above its junction with the Mississippi, is a nearly level expanse of till, only moderately undulating, with no prominent hills or notable depressions, excepting this deep channel and those formed by its tributary streams. Below Carver it intersects a belt of terminal moraine, composed of hilly till. Its entire course is through a region of unmodified drift, which has no exposures of solid rock upon its surface.

Bluffs in slopes from twenty to forty degrees, and rising one hundred to two hundred feet to the general level of the country, form the sides of this trough-like valley. They have been produced by the washing away of their base, leaving the upper portions to fall down and thus take its steep slopes. The river in deepening its channel has been constantly changing its course, so that its current has been turned alternately against the opposite sides of its valley, at some time undermining every portion of them. In a few places this process is still going forward, but mainly the course of the Minnesota River is in the bottomland. Comparatively little excavation has been done by the present river. As we approach its source it dwindles to a small stream flowing through long lakes, and we finally pass to Lake Traverse, which empties northward; yet along the upper Minnesota and at the divide between this and the Red River, this valley or channel and its enclosing bluffs are as remarkable as along the lower part of the Minnesota River. It is thus clearly shown to have been the channel of outflow from a lake formerly extending northward from Lake Traverse along the Red River valley. The existence of this body of water is believed to have been due to the barrier of the receding continental ice-sheet, obstructing the natural course of drainage northward as at present to Hudson Bay; and it is therefore called *Lake Agassiz*, in memory of Professor Louis Agassiz, the first prominent advocate of the theory that the drift was produced by land-ice. The shore line of this lake, when it stood at its highest level, marked by a distinct beach of gravel and sand, upon a surface which on each side of this beach is chiefly till, has been traced continuously one hundred and seventy-five miles, from Lake Traverse easterly to Herman and thence northerly to Maple lake, twenty miles east-southeast from Crookston. The height of this beach has been ascertained at the same time, by leveling along this entire distance.

The Minnesota valley in many places cuts through the sheet

of drift and reaches the underlying rocks, which have frequent exposures along its entire course below Big Stone Lake. This excavation shows that the thickness of the general drift-sheet upon this part of Minnesota averages about one hundred and fifty feet. The contour of the old rocks thus brought into view is much more uneven than that of the drift. In the hundred miles from Big Stone Lake to Fort Ridgely the strata are metamorphic gneisses and granites, which often fill the whole valley, one to two miles wide, rising in a profusion of knolls and hills, fifty to one hundred feet above the river. The depth eroded has been limited here by the presence of these rocks, among which the river flows in a winding course, crossing them at many places in rapids or falls. From New Ulm to its mouth the river is at many places bordered by Cretaceous and Lower Silurian rocks, which are nearly level in stratification. These vary in height from a few feet to fifty or rarely seventy-five or one hundred feet above the river. From Mankato to Ottawa the river occupies a valley cut in Shakopee limestone underlain by Jordan sandstone, which form frequent bluffs upon both sides, fifty to seventy-five feet high. After excavating the overlying one hundred and twenty-five to one hundred and fifty feet of till, the river here found a former valley, eroded by pre-glacial streams. Its bordering walls of rock, varying from one-fourth of a mile to at least two miles apart, are in many portions of this distance concealed by drift, which alone forms one or both sides of the valley. The next point at which the river is seen to be enclosed by rock-walls, is in its last two miles, where it flows between bluffs of Trenton limestone underlain by St. Peter sandstone, one hundred feet high and about a mile apart. This also is a pre-glacial channel, its farther continuation being occupied by the Mississippi River. The only erosion effected by the Minnesota River here has been to clear away a part of the drift with which the valley was filled. Its depth at some earlier time was much greater than now, as shown by the salt-well on the bottomland of the Minnesota River at Belle Plaine, where two hundred and two feet of stratified gravel, sand and clay were penetrated before reaching the rock. The bottom of the pre-glacial channel there is thus at least one hundred and sixty-five feet lower than the mouth of the Minnesota River.

Heights of the bluffs, which form the sides of this valley, composed of till enclosing layers of gravel and sand in some places, and frequently having rock at their base, are as follows, stated in feet above the lakes and river: along Lake Traverse, 100 to 125; at Brown's Valley and along Big Stone Lake, mainly about 125, the highest portions reaching 150; at Ortonville, 130; at Lac qui Parle and Montevideo, 100; at Gran-

ite Falls, 150; at Minnesota Falls, 165; thence to Redwood Falls, Fort Ridgely and New Ulm, 165 to 180; at Mankato, 200 to 225; at Saint Peter and Ottawa, 220 to 230; at Le Sueur and Henderson, 210 to 225; at Belle Plaine and Jordan, about 230; and at Shakopee, 210 to 220. The morainic hills through which this valley extends below Shakopee are 225 to 250 feet in height. The expanse of till through which this channel is eroded slopes from 1125 feet above the sea at Big Stone Lake to 975 at Mankato, in 140 miles; and thence it descends to 925 at Shakopee, in 50 miles. This channel or valley of the Minnesota River lies nearly midway between the belt, on its northeast side, of medial and terminal moraines, that extends from Lake Minnetonka 150 miles northwest to the Leaf hills, and the Coteau des Prairies on its southwest side; toward each of which, some fifty miles distant from this river, there is a gentle ascent, sufficient to cause drainage to follow this central line.

The height of Lake Traverse is 970 feet above the sea; the lowest point in Brown's Valley between this and Big Stone Lake is only three feet above Lake Traverse; Big Stone Lake is 962 feet above the sea, or eight feet below Lake Traverse; and the mouth of the Minnesota River is 690 feet above the sea, the descent from Big Stone lake to the mouth of the river being 272 feet.

Lakes Traverse and Big Stone are from one to one and a half miles wide, mainly occupying the entire area between the bases of the bluffs, which rise about one hundred and twenty-five feet above them. Lake Traverse is fifteen miles long; it is mostly less than ten feet deep, and its greatest depth probably does not reach twenty feet. Big Stone Lake is twenty-six miles long, and its greatest depth is reported to be from fifteen to thirty feet. The portion of the channel between these lakes is widely known as Brown's Valley. As we stand upon the bluffs here, looking down on these long and narrow lakes in their trough-like valley, which extends across the five miles between them, where the basins of Hudson Bay and the Gulf of Mexico are now divided, we have nearly the picture which was presented when the melting ice-sheet of British America was pouring its floods along this hollow. Then the entire extent of the valley was doubtless filled every summer by a river which covered all the present areas of flood-plain, in many places occupying as great width as these lakes.

General Warren observed that Lake Traverse is due to a partial silting up of the channel since the outflow from the Red River basin ceased, the Minnesota River at the south having brought in sufficient alluvium to form a dam; while Big Stone Lake is similarly referred to the sediment brought into

the valley just below it by the Whetstone River. Fifteen miles below Big Stone Lake, the Minnesota River flows through a marshy lake four miles long and about a mile wide. This may be due to the accumulation of alluvium brought into the valley by the Pomme de Terre River, which has its mouth about two miles below. Twenty-five miles from Big Stone Lake, the river enters Lac qui Parle, which extends eight miles with a width varying from one-fourth to three-fourths of a mile and a maximum depth of twelve feet. This lake, as General Warren suggested, has been formed by a barrier of stratified sand and silt which the Lac qui Parle River has thrown across the valley. He also showed that Lake Pepin on the Mississippi is dammed in the same way by the sediment of the Chippewa River; and that Lake St. Croix and the last thirty miles of the Minnesota River are similarly held as level back-water by the recent deposits of the Mississippi.

All the tributaries of the Minnesota River have cut deeply into the drift, because the main valley has given them the requisite slope. The largest of these extend many miles, and have their mouths level with the bottomland of the Minnesota River. The bluffs of all these valleys are also everywhere seamed and gullied by frequent rills and springs, many of which flow only after rains. Few of the large inlets have any great amount of sediment deposited opposite their mouths, showing that their excavation was mostly done at the same time with that of the main valley. The short ravines are more recent in their origin, and the material that filled their place is commonly spread in fan-shaped, moderately sloping banks below their mouths, which are thus kept at a height from thirty to forty feet above the present flood-plain. The road from Fort Ridgely to New Ulm runs along the side of the bluff at the only height where a nearly level straight course could be obtained, being just above these deposits and below the ravines.

The valleys of the Pomme de Terre and Chippewa Rivers, 75 to 100 feet deep along most of their course, and one-fourth of a mile to one mile in width, were probably avenues of drainage from the melting ice-fields in their northward retreat. Between these rivers, in the twenty-two miles from Appleton to Montevideo, the glacial floods at first flowed in several channels, which are excavated forty to eighty feet below the general level of the drift-sheet, and vary from an eighth to a half of a mile in width. One of these, starting from the bend of the Pomme de Terre River, one and a half miles east of Appleton, extends fifteen miles southeast to the Chippewa River near the center of Tunsburg. This old channel is joined at Milan station by another, which branches off from the Minnesota valley, running four miles east-southeast; it is also joined at the northwest

corner of Tunsburg by a very notable channel which extends eastward from the middle of Lac qui Parle. The latter channel, and its continuation in the old Pomme de Terre valley to the Chippewa River, are excavated nearly as deep as the channel occupied by the Minnesota River. Its west portion holds a marsh generally known as the "Big Slough." Lac qui Parle would have to be raised only a few feet to turn it through this deserted valley. The only other localities where we have proof that the outflow from Lake Agassiz had more than one channel are seven and ten miles below Big Stone Lake, where isolated remnants of the general sheet of till occur south of Odessa Station and again three miles southeast. Each of these former islands is about a mile long, and rises seventy-five feet above the surrounding low land, or nearly as high as the bluffs enclosing the valley, which here measures four miles across, having a greater width than at any other point.

Terraces and high plains of modified drift, found in many places along the valley of the Minnesota River from New Ulm to its mouth, show that it was once filled, doubtless at the close of the last glacial epoch, with stratified gravel, sand and clay, to a depth 75 to 150 feet above the present river. The remnants of this deposit include the plateau of modified drift, about a mile long and an eighth of a mile wide, upon which the west and highest part of New Ulm is built; a terrace in section 27, Courtland, opposite the southeast part of New Ulm, more than a mile long and about an eighth of a mile wide; a larger terrace, four miles long and a half mile wide, lying also in Courtland, four to eight miles southeast from New Ulm, upon which Courtland depot is situated; a terrace extending about three miles northwest from near Minneopa falls, and varying from a few rods to a third of a mile in width; a terrace three miles long east and south of Kasota; the "Sand prairie," about four miles long and averaging a mile wide, west and north of Saint Peter; Le Sueur prairie, six miles long and from one to three miles wide, beginning east of Ottawa and reaching to Le Sueur; the plain five miles long and a mile wide, near the middle of which Belle Plaine is built; Spirit hill and "Sand prairie," southwest and northwest of Jordan; a terrace eight miles long and varying from a few rods to two miles in width, extending through San Francisco, Dahlgren and Carver; and Shakopee prairie, eight miles long and averaging one mile wide. The height of these terraces and plains at New Ulm is about 115 feet above the river; in Courtland, and near Minneopa falls, 125 to 150 feet; at Kasota, Saint Peter and Le Sueur, about 150 feet; at Belle Plaine, about 135; and at Jordan, Carver and Shakopee, about 125. Wells on the "Sand prairie" near Saint Peter and on Le Sueur prairie

go through sand and gravel, sometimes with layers of clay, to the depth of 75 or 100 feet, finding till below. At Belle Plaine the sand and gravel are about fifty feet deep, underlain by till. Shakopee prairie has forty or fifty feet of this modified drift, lying upon limestone. The principal remnant of these deposits seen below Shakopee was a terrace about seventy-five feet high, an eighth to a third of a mile wide and four miles long, extending through Eagan in Dakota county, its north end being about two miles south of Fort Snelling. This valley was first excavated in till which rises in continuous bluffs on each side 50 to 100 feet above these high plains and terraces of modified drift. It was afterward filled along this distance of one hundred miles next to its mouth with fluvial deposits 75 to 150 feet thick, sloping about two feet per mile, through which the channel has been cut anew.

With this general view of the valley of the Minnesota river as it exists to-day, we are prepared to inquire more particularly what its history was during the vicissitudes of the ice age and from the close of that period until now. Its condition at the beginning of the reign of ice first claims consideration. During what ages was its pre-glacial rock-walled channel formed? This is in part answered by deposits of Cretaceous clay found in water-worn hollows of the Shakopee limestone forming the walls of this channel at numerous places in Blue Earth, Le Sueur and Nicollet counties, and by the occurrence in this valley in Courtland and New Ulm and near Fort Ridgely and Redwood Falls, of Cretaceous sandstone, clay and shale, occasionally containing lignite. It is thus known that before the Cretaceous age, when Western Minnesota and the region of the upper Missouri were depressed and covered by the sea, a deep channel had been cut by some river in the Lower Magnesian strata of the Minnesota valley. The slopes from each side toward this channel appear therefore to have been partly like those of the present day. The direction of drainage may even have been the same as now, for the northern part of the Mississippi River and valley has probably existed since the middle of Paleozoic time.

Scanty exposures of Cretaceous strata are found in many parts of Minnesota, enclosing sometimes marine shells, sometimes impressions of leaves, and at a few places thin layers of lignite. The western two-thirds of the State were probably covered until the glacial period by deposits of this age which have now been mainly eroded, with much from the underlying Paleozoic rocks, and constitute a part of the drift, irrecognizably mingled with detritus and bowlders that have been brought from Laurentian and Huronian areas far to the north and northeast. Excepting its partial submergence by the sea in the Cre-

taceous age, Minnesota seems to have stood wholly above the level of the ocean from the beginning of the Carboniferous period till the present day.

Before the ice age the rocks had been long subjected to the ordinary disintegrating agencies of rain and frost. Granite and gneiss were thus generally decomposed in place to a considerable depth. The loose material resulting from such decay was doubtless spread somewhat evenly over the surface, collecting to the greatest depth in valleys. Except where it had been transported by streams and consequently formed stratified deposits, the only fragments of rock held in this mass would be from underlying or adjoining rocks.

During the ice age the Cretaceous strata, mostly unconsolidated, which covered Western Minnesota, the superficial deposits and decomposed rock produced by weathering, and the alluvial beds of sand and gravel along the river-courses, were mostly plowed up by the ice and thoroughly kneaded with each other. Much detritus was also added from erosion of the harder rocks beneath. Large blocks and bowlders, often already formed by the process of weathering, were borne away, and the surface of the bed-rock was worn and striated by bowlders and pebbles, which were rolled and dragged along under the vast weight of ice, breaking up and grinding themselves and the underlying rock into gravel, sand and even the finest clay.

Ledges of decomposed gneiss and granite, found in the Minnesota valley at many points from Minnesota Falls to Fort Ridgely, with their surface changed to a soft earthy or clayey mass, resembling kaolin, and the Cretaceous beds before mentioned, both of which would readily yield to eroding agencies, show that the moving ice-sheet did not everywhere plow up all the loose material under it. A considerable depth, however, has probably been removed; and these may be scanty remnants of thick beds which covered this region generally before the glacial period. More commonly the ice-sheet removed all such material, and gathered a part of its drift from the underlying solid rocks; as is shown by their being frequently rounded, smoothed and marked with parallel furrows and scratches, called striæ. Similarly scratched pebbles and bowlders are found in the glacial drift. These were the graving tools by which the bed-rock was worn and striated. They were held firmly by being frozen in the bottom of the ice and were pushed forward by its current, which thus recorded its direction. Our observations of glacial striæ in the Minnesota valley are as follows: one to three miles southeast from the foot of Big Stone Lake, S.E.; near Odessa, S.E.; at Granite Falls, in several places, S. 45°–50° E.; at the dam of O. K. mill, Beaver Falls, S. 60° E.; one and a half miles west from Fort

Ridgely, S. 60° E.; at Redstone, one and a half miles southeast from New Ulm, S. 25° E.; at Jordan, noted by Foss, Wells & Co., in quarrying and on the site of their mill, S.E.

Within the till are frequently found layers of sand or gravel, which yield the large supplies of water so often struck in digging wells. Probably many of these veins of modified drift were formed by small sub-glacial streams and therefore cannot be regarded as marking divisions of the glacial period, nor even any important changes in the overlying ice. It appears, however, by shells, remains of vegetation, and trees, found deeply buried between glacial deposits in this and adjoining States, that the ice age was not one unbroken reign of ice, but that this retreated and re-advanced, or was possibly at some time nearly all melted upon the northern hemisphere and then accumulated anew. Thus periods of ice alternated with interglacial epochs, in which animal and vegetable life spread again northward, following close upon the retreat of the ice-fields. By each new advance of the glacial sheet much of the previous surface would be ploughed up and re-deposited; hence we find only few and scanty remnants of fossiliferous beds in the glacial drift. At the disappearance of the last ice-sheet these drifted materials, seldom modified by water in their deposition, formed a mantle 100 to 200 feet thick, which throughout the basin of the Minnesota River almost universally covered the older rocks.

[To be concluded.]

ART. VIII. — *On the so-called Dimorphism in the Genus Cambarus;* by WALTER FAXON.

THE existence of two forms of the adult male in all the species of the genus *Cambarus* was discovered by Louis Agassiz and Henry James Clark. The differences between the two forms affect more especially the first pair of abdominal appendages, organs concerned in the act of coition, but also extend to the general form and sculpture of the body. In one form (unhappily called by Dr. Hagen the "second form"), the first pair of abdominal appendages have a structure nearly like that seen in all *young* males. The hooks on the third joint of the third (in some species of the third and fourth) pair of legs are small, and in the sculpture of the shell and shape of the claws, this form approaches the female. In the other form (Hagen's "first form"), the articulation near the base of the first pair of abdominal appendages is gone and the whole member is much more highly specialized, the terminal hooks being horny, more widely separated and in every way more highly developed; in

those species with bifid tips to these appendages, the branches are longer, slenderer, more widely separated and stiffer; the hooks on the thoracic legs are longer and more perfectly finished; the sculpture of the whole body is more pronounced and the claws are larger and more powerful. No intermediate conditions are found, and there is no relation between these forms and the size of the individual, the "second form" being large and the "first form" small, or *vice versa*. Hence we are forbidden to interpret the two forms as stages in ordinary development. Dr. Hagen has shown that in individuals of the "second form" the internal generative organs are smaller than in the "first form," but having only alcoholic material he was unable to determine anything concerning the presence or absence of spermatozoa. He interprets the facts as a case of dimorphism and surmises that the "second form" males are sterile individuals.

In the autumn of 1875, I received a lot of living *Cambarus rusticus* Girard, from Kentucky, males of the "first form" and females, which bred freely in confinement. After pairing, three of the males moulted and were thrown, while in the soft-shelled state, into alcohol together with their exuviæ. An examination of these specimens now reveals the fact that the soft-shelled specimens are all of the "second form," their exuviæ of the "first form!" After attaining the "first form" and after pairing, the same individual has reverted to the "second form." It is now clear that we are not dealing with a case of true dimorphism such as is well known among insects and plants, but it appears probable that the two forms of the crayfish are alternating periods in the life of the individual, the "first form" being assumed during the pairing season, the "second form" during the intervals between the pairing seasons. It is to be inferred that before the animal is again capable of reproduction, another moult will bring it again into the "first form."

The fact that large collections, made at one time and place, often contain only one or a great preponderance of one, form of the male, is now explained.

I have also before me a male specimen of *Cambarus propinquus* Girard, from Wisconsin, belonging to the Peabody Museum of Yale College, which was taken in the act of moulting. The old shell is "first form," the soft shell emerging from it is "second form."

It is remarkable that two forms of the male have not been detected in any other genus of crayfishes.

Fritz Müller (Für Darwin) has pointed out the existence of two forms of the male in the genera *Tanais* and *Orchestia* which he considers as truly dimorphic forms. It is possible that these are to be explained in the same way as the two forms of the male *Cambarus*.

Such a change as this connected with the reproductive periods is unparalleled, so far as I know, among the Invertebrata, and even among the Vertebrata; the cases of partial atrophy of the generative organs or shedding of antlers (as in the stag) after the rut is over are hardly comparable.

At the time I had the specimens alive my attention had not been drawn to the questions relating to the two forms of the males, so that I failed to make anatomical examination, and the specimens have now lain too long in alcohol to be serviceable for internal dissection. I hope, however, that naturalists who are more favorably situated will be able to throw more light on this subject.

I will add that the males of extraordinary size which I have seen, are all of the "first form." Do these very old individuals cease to moult? Do they become permanently capable of reproduction?

Museum of Comparative Zoölogy, Cambridge, Mass., Nov. 12, 1883.

Art. IX.—*Evolution of the American Trotting Horse;* by Francis E. Nipher.

In the November number of this Journal, Mr. W. H. Pickering has criticised the method of reduction used in my paper in the July number, and has reached a conclusion very different from my own. I wish to discuss his criticisms briefly.

Mr. Pickering thinks it objectionable to determine the value of $\frac{ds}{dT}$ or the change in speed per year, by taking alternate differences in s and T, and he has reduced the observations by taking the differences between consecutive values in the table. In this way he gets the values in the third column in the table below.

s	Year.	$\frac{ds}{dT}$ obs.	$\frac{ds}{dT}$ calc.	E.
145	1854·0			
		0·59	0·59	0·0
143	1857·4			
		0·55	0·56	−0·1
141	1861·0			
		0·54	0·54	0·0
139	1864·7			
		0·46	0·51	−0·4
137	1869·0			
		0·55	0·49	+0·4
135	1872·6			
		0·35	0·46	−1·4
133	1878·3			
		0·75	0·44	+1·8
131	1881·0			

Plotting the values of $\frac{ds}{dT}$ and the corresponding values of s, he then goes on to say, that the points so determined may be represented by a curve such that the value of $\frac{ds}{dT}$ increases when that of s diminishes. Assuming as I had done, that a straight line will represent the values, he determines the values of the constants, and finds that the line intersects the s axis at a point where s is -25. This would mean that the horse would finally trot a mile in less than no time.

When making the first discussion of the subject, the writer considered the propriety of determining $\frac{ds}{dT}$ by means of consecutive differences, and unfortunately rejected the method without even giving it a trial, for the reason, that the dates 1881·0 and 1878·3 were very imperfectly determined. It was clear that the additional point gained would deserve very little weight. It was thought best to smooth the line by combining these with previous and better determined dates. Properly used, one of these methods is probably as reliable as the other. But when Mr. Pickering's method is used, it becomes absolutely necessary to take into account the fact, that the different values of $\frac{ds}{dT}$ are determined with very different degrees of precision, Mr. Pickering has given them all equal weight, and this is the fatal defect which, it seems to me, entirely vitiates the conclusion reached by him. A reference to fig. 1 of my paper in the July number, will show that for the earlier dates, from 1854·0 down to 1872·6 the graphically determined dates differ from the real dates when the record was actually lowered, by from one to two years. It will also be seen that the dates 1878·3 and 1881·0 are subject to errors which may be as great as two years.

After having made a preliminary examination, these dates might indeed have been "adjusted," so as to make them agree better with the others, and this without giving a "cooked" appearance to the reduction; but they now stand exactly as they did when first determined and before any other work had been done. I have plotted the new values of $\frac{ds}{dT}$ with the values of s, and the line representing the values so as to give most weight to the best determined values, I find to be represented by the equation

$$\frac{ds}{dT} = -1{\cdot}24 + 0{\cdot}0127\ s.$$

This line is nearly coincident with the line marked A A in the diagram in Mr. Pickering's paper.

From this equation the values of $\frac{ds}{dT}$ were calculated as given in the fourth column of the table above. The fifth column, headed E, gives the time in years by which the corresponding time intervals dT must be increased, in order to bring Mr. Pickering's values of $\frac{ds}{dT}$ of the third column, into accordance with the values calculated from the above equation. In this case the intervals were supposed to be separately adjusted. If the later dates were simultaneously adjusted by intervals ranging from a quarter to three-fourths of a year, the values which Mr. Pickering prefers to use would agree exactly with the values calculated from the last equation. Now it is perfectly clear, that these later dates, and particularly the last two, are subject to just such errors as this. If the date 1881·0 were made 1882·8, the value of $\frac{ds}{dT}$ instead of being 0·75 would be 0·44.

Whatever these values of $\frac{ds}{dT}$ may be said to prove, therefore, they do not prove that my results as before published were absurd, and they do not indicate a limiting speed of one mile in 25 seconds less than no time, but when $\frac{ds}{dT}=0$ the value of s from the last equation is 98 seconds.

I desire to express my thanks to Mr. Pickering for his suggestion and his friendly criticism, as he has corrected a tendency which I had begun to feel, to attach too much importance to the numerical results reached; but I maintain that his method, correctly applied, gives in general, substantially the same result as my own. It is not necessary to assert that this result is really correct, if any person feels inclined to doubt it. I only insist that the conclusion that the trotting horse will finally trot his mile in about the same time that the running horse will run, is not unwarranted by the facts which we now know.

Most horsemen seem to think that the limiting speed of the trotting horse will be somewhere near a mile in 120 seconds. If this were true, the differential equation could hardly be a linear one. The equation

$$\frac{ds}{dT}=K\sqrt{s-L}$$

might, however, represent the values, L being the limiting

value of s. But this equation gives on integration an equation of the form,

$$\sqrt{s-L}=C-AT.$$

According to this equation the horse would absolutely reach the limiting speed in a finite time $\frac{C}{A}$. Practically this may be true, as is in fact shown by my own equation (4) in the July number, so that some such equation might really represent the results sufficiently for all practical purposes. But the relation is not a rational one, since it cannot be supposed that the horse will really attain his limiting speed in a finite time. After he had come within a thousandth of a second, it would take a mighty effort, and a great interval of time to compass the next millionth of a second.

Moreover this equation will not hold after the limiting space shall have been attained, since it is the equation of a parobola, and the value of s will then begin to increase, which is evidently absurd.

Washington University, Nov. 10, 1883.

ART. X.—*On the Origin of Jointed Structure;* by G. K. GILBERT.

IN the number of this Journal for July, 1882, the writer pointed out that existing theories of the origin of joint structure were unsatisfactory. His note appears to have excited interest and borne fruit. Mr. McGee, in the February number of the Journal, has made a suggestion on the subject, and Messrs. Walling and Crosby have elsewhere published theories.

Mr. McGee suggests "that the vertical compression and consequent lateral expansion of beds beneath areas of deposition may produce incipient slaty cleavage along certain lines perhaps determined by crystalline structure, and that the vertical expansion and consequent lateral contraction of the same beds, when lightened by denudation and subjected to cooling and desiccation, may develop such lines in the jointage planes."

It can scarcely be questioned that incipient joints are developed into actual joints and open crevices by contraction due to cooling and desiccation; but it seems hardly admissible to compare the initiatory process as conceived by Mr. McGee to slaty cleavage. In slaty cleavage division planes are normal to the direction of pressure, while under this hypothesis they are parallel to that direction.

Mr. Walling and Mr. Crosby independently propose the theory that jointed structure is produced by earthquakes. Mr.

Walling's paper appears in the proceedings of the Montreal meeting of the American Association for the Advancement of Science. Mr. Crosby's, which is the more elaborate, was read to the Boston Society of Natural History in October, 1882.

Mr. Crosby shows, first, that the earthquake vibration, by subjecting the imperfectly elastic material through which it passes to alternate stress and tension, is competent to produce fractures; second, that such fractures would be normal to the direction of wave propagation, and therefore vertical, except in the immediate vicinity of the earthquake focus, where they might be oblique; third, that such fractures, although actually curved, would have generally so great a radius of curvature as to be sensibly plane; fourth, that they would be parallel; and fifth, that the suddenness of the earthquake shock would tend to produce smooth fractures even in heterogeneous material. All these points sustain the hypothesis, and their collective effect is to give it great strength. There are, however, two features of joint structure with which the theory does not *prima facie* consist.

In the first place the angle of intersection of two co-existent systems of joints is usually high. This is recognized as a difficulty by Mr. Crosby, and he says in explanation, "that, after the rocks have been broken by one set of joints, the layers or sheets thus formed possess a strong natural tendency to break at right angles; and, under such circumstances, oblique vibrations may give rise to rectangular fractures and blocks." If I rightly understand him, he refers by the expression, "strong natural tendency," to the comparative ease with which an elongated body of amorphous material may be broken in the direction of its least diameter, a property evidently dependent on the fact that fracture through the smallest diameter involves the overcoming of cohesion through a surface of minimum area. If this were the true explanation of the high angle assumed by secondary joints, each of the layers between primary joints would be affected independently and the planes of cross-jointing would be discontinuous. With continuous planes of cross-jointing, the total cohesion overcome is not diminished by any modification of attitude.

A somewhat different explanation has occurred to me as possible. It is, that after one set of joints has been established normal to an original direction of earthquake impulse, a second impulse, somewhat oblique to the first, may have its tensions relieved by the existing joints, and for this reason fail to produce a new set. An impulse at right angles to the original direction would produce tensions not at all relieved by the existing joints, and would initiate a series dividing the terrane into rectangular blocks. The defect of this explanation is that

in diminishing one difficulty it increases another, as will presently appear.

The second feature of joint structure which occasions difficulty with the earthquake theory is that the angle of intersection of coexistent systems is *sometimes* very small. Mr. Jukes, in his Manual, mentions 5° as a measured angle; and, without being able to cite measurements, the writer believes he has observed angles as small as that. The considerations set forth in the preceding paragraph sufficiently explain how this phenomenon constitutes a difficulty.

If these two features can be satisfactorily explained, there seems no bar to the substitution of the earthquake theory for those previously entertained.

ART. XI. — *A Theory of the Earthquakes of the Great Basin, with a practical application;* by G. K. GILBERT.

[From the Salt Lake Tribune of Sept. 20, 1883.]

THERE are many geologists who are very wise, but even they do not understand the forces which produce mountains. And yet it must be admitted, not only that mountains have been made, but that some mountains are still rising. The mysterious forces appear to act in different ways in different places, and it is possible that their nature is not universally the same. Suffice it to say that in the Great Basin the movements they cause are vertical. It is as though something beneath each mountain was slowly, steadily, and irresistibly rising, carrying the mountain with it.

In yielding to this all-compelling upward thrust, the earth's crust sometimes bends and stretches, but more often it breaks; and when it breaks, the fracture occurs in a peculiar place. It does not run along the medial axis of the mountain, but along one margin. On one side of the fracture the crust is lifted and tilted; on the other side it either sinks or remains undisturbed. The uplifted part of the crust is the mountain, and the storms carve out its cañons; the unlifted part remains a lowland or valley, and receives the debris washed out from the cañons.

A mountain is not thrown up all at once by a great convulsive effort, but rises little by little. The subterranean upthrust is continuous and slow, and would produce a continuous upward movement of the mountain if the mountain's weight were the only resisting factor. But there is also a great friction to overcome, the friction along the surface of fracture between the rising and stationary parts of the crust; and fric-

tion gives to slow motion an uninterrupted or rhythmic character.

The disagreeable jarring of a railway car while the brake is set is due to the interruption of motion by friction, the wheels alternately sliding and stopping. The musical vibration of a violin string is due to the alternate cohesion and sliding of the bow upon it, and fails when the friction of the bow is insufficient. Attach a rope to a heavy box and drag it slowly, by means of a windlass, across a floor. As the crank is turned, the tension of the rope gradually increases until it suffices to overcome the starting friction, as it is called. Once started, the box moves easily, because sliding friction is less than starting friction. The rope shortens or sags until its tension is only sufficient for the sliding friction, and it would continue in that state but that the box, having acquired momentum, is carried a little too far. This slacks the rope still more, and the box stops, to be started only when the tension again equals the starting friction. In this way the box receives an uneven, jerky motion.

Something of this sort happens with the mountain. The upthrust produces a local strain in the crust, involving a certain amount of compression and distortion, and this strain increases until it is sufficient to overcome the starting friction along the fractured surface. Suddenly, and almost instantaneously, there is an amount of motion sufficient to relieve the strain, and this is followed by a long period of quiet, during which the strain is gradually reimposed. The motion at the instant of yielding is so swift and so abruptly terminated as to constitute a shock, and this shock vibrates through the crust with diminishing force in all directions. Movable objects are displaced, and the soil, which is movable as compared with solid rock, is cracked. In consequence of earth cracks, subterranean waters find new channels, leading to the stoppage of some springs and the starting of others. In fine, all the phenomena of an earthquake are produced.

This is not a universal theory of earthquakes—some of them are doubtless to be accounted for in a different way; but it affords a sufficient, and I do not doubt that it affords the true, explanation of the earthquakes of the Great Basin. In this region a majority of the mountain ranges have been upraised by the aid of a fracture at one side or the other, and in numerous instances there is evidence that the last increase of height was somewhat recent.

Let us look a moment at this evidence. The material eroded from a mountain by the elements is washed out through the cañons and deposited in the adjacent valleys. The coarser part of it lodges at the mountain base, and is built into a

sloping mass called the foot-slope, or colloquially the "bench." When an earthquake occurs, a part of the foot-slope goes up with the mountain, and another part goes down (relatively) with the valley. It is thus divided, and a little cliff marks the line of division. A man ascending the foot-slope encounters here an abrupt hill, and finds the original grade resumed beyond. This little cliff is, in geologic parlance, a "fault-scarp," and the earth fracture which has permitted the mountain to be uplifted is a "fault." In the course of time the same slow process of erosion and deposition which originally formed the foot-slope restores its shape and obliterates the fault-scarp. When a mountain ceases to grow, its fault-scarp soon disappears; and conversely, when we find a fault-scarp at the base of a mountain, we are assured that the uplifting force has not ceased to act. Fault-scarps have now been found at the bases of so many ranges of the Great Basin, that it is safe to say that the subterranean forces are generally active in this region, and this is especially true of all the large mountain masses. The Wasatch is a conspicuous example, and residents of this city need not go far for ocular demonstration. A fault-scarp, thirty or forty feet high, divides the powder houses north of the Hot Spring, so that some of them stand above and some below it, and considerable grading was necessary to lead the road to the upper magazines. With one exception, all the lime kilns between the powder houses and the Warm Springs are built in the face of the fault-scarp, the lime rock being conveniently delivered to the kilns from the upper level, and the lime as conveniently drawn out at the lower level. At the mouth of Little Cottonwood Cañon, a smelter has been built on the edge of the upper bench for the convenience of dumping its slag over the fault-scarp. At the mouth of Spanish Fork Cañon, the D. & R. G. Railroad encounters the scarp, and the engineers have started an embankment a long way back to climb it. Similar features may be seen, with rare intervals, all along the mountain base from Nephi to Willard.

The fault-scarps of the Wasatch follow the western base. Those of the Sierra Nevada follow the eastern base; and it happens that one of them has been formed since the settlement of the country. It occurred in 1872, and produced one of the most notable earthquakes ever recorded in the United States. The height of the scarp varies from five to twenty feet, and its length is forty miles. Various tracts of land were sunk a number of feet below their previous positions, and one tract, several thousand acres in extent, was not only lowered, but carried bodily about fifteen feet northward. The ground was cracked in various directions, and several springs permanently disappeared. All houses of adobe or stone in the immediate

vicinity were thrown down, and about thirty persons lost their lives. In the little town of Lone Pine, numbering some three hundred inhabitants, twenty-one were killed by falling walls.

There was only one violent shock, and the damage was all done in a few seconds, but for two months there were occasional tremors. Theoretically, the main strain of the earth's crust was relieved at once, but a complete equilibrium was brought about more slowly.

The surviving inhabitants of Lone Pine observed that the only houses that remained standing were of wood, and in rebuilding they employed that material exclusively. Such a course was natural, but I conceive that their precaution was unnecessary. They may, indeed, feel feeble shocks propagated from earthquakes centering elsewhere, but in their own locality the accumulated earthquake force is for the present spent, and many generations will probably pass before it again manifests itself. The old maxim, "Lightning never strikes the same spot twice," is unsound in theory and false in fact; but something similar might truly be said about earthquakes. The spot which is the focus of an earthquake (of the type here discussed) is thereby exempted for a long time. And conversely, any locality on the fault line of a large mountain range, which has been exempt from earthquake for a long time, is by so much nearer to the date of recurrence—and just here is the application of what I have written. Continuous as are the fault-scarps at the base of the Wasatch, there is one place where they are conspicuously absent, and that place is close to this city. From the Warm Springs to Emigration Cañon fault-scarps have not been found, and the rational explanation of their absence is that a very long time has elapsed since their last renewal. In this period the earth strain has been slowly increasing, and some day it will overcome the friction, lift the mountains a few feet, and re-enact on a more fearful scale the catastrophe of Owens Valley.

It is useless to ask when this disaster will occur. Our occupation of the country has been too brief for us to learn how fast the Wasatch grows; and, indeed, it is only by such disasters that we can learn. By the time experience has taught us this, Salt Lake City will have been shaken down, and its surviving citizens will have sorrowfully rebuilt it of wood; to use a homely figure, the horse will have escaped, and the barn door, all too late, will have been closed behind him.

When the earthquake comes, the severest shock is likely to occur along the line of the great fault at the foot of the mountain. This line follows the upper edge of the upper bench from Big Cottonwood Cañon to the rifle targets back of Fort Douglass, cutting across each creek just where it issues from

between walls of bed-rock, and passing only a short distance back of the fort. At a point not far north of the targets, the fault divides; one branch continuing northward, across the spur, toward Farmington; the other turning westward, running just back of that hopeless artesian boring, and following the upper edge of the gravel bench to the vicinity of the Warm Springs. Should the earthquake follow the former of these branches, the city will not fare so badly as the fort; should it follow the latter, or follow both, city and fort will alike suffer severely.

What are the citizens going to do about it? Probably nothing. They are not likely to abandon brick and stone and adobe, and build all new houses of wood. If they did, they would put themselves at the mercy of fire; and fire, in the long run, unquestionably destroys more property than earthquakes. It is the loss of life that renders earthquakes so terrible. Possibly some combination of building materials will afford security against both dangers.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the constitution of Bleaching Powder.*—The composition of bleaching powder has long been a subject of discussion. The original formula of Gay Lussac corresponds to $Ca(OCl)_2 + CaCl_2$, while Odling gives the formula Cl–Ca–OCl and Stahlschmidt the formula HO–Ca–OCl. O'Shea has investigated the matter anew in a way which throws much light upon it. He distinguishes at the outset between bleaching powder as a whole and the special oxidizing compound contained in it which he calls the bleaching compound. That the molecule of the latter contains ClO has been placed beyond doubt by the researches of Kopfer. The question to be solved is whether the elements of water or of calcium chloride enter in any way into the molecule. Stahlschmidt represents the formation of bleaching powder thus:

$$\left(Ca\left\{\begin{matrix}OH\\OH\end{matrix}\right\}\right)_2 + \left(\begin{matrix}Cl\\Cl\end{matrix}\right\}\Big)_2 = \left(Ca\left\{\begin{matrix}OH\\OCl\end{matrix}\right\}\right)_2 + Ca\left\{\begin{matrix}Cl\\Cl\end{matrix}\right. + (H_2O)_2$$

Hence it must contain free calcium chloride. Odling's formula pre-supposes the non-existence of free calcium chloride. The author sought to determine therefore, (1) the proportion of $Ca(ClO)_2$, of $CaCl_2$, and of $Ca(OH)_2$ in dry bleaching powder, (2) the existence of free $CaCl_2$ and (3) the composition of the residue after removal of the $CaCl_2$. Pure lime was hydrated and analyzed. It yielded from 21·27 to 39·02 per cent of water in different samples. It was then exposed to chlorine until no further increase in

weight was observed. In order to fix the ratio of molecules, the total lime was precipitated as oxalate, the total chlorine determined by boiling with ammonia and precipitation with silver nitrate, the available chlorine by Bunsen's method, the calcium hydrate by boiling with ammonia, evaporating to dryness, extracting with alcohol, and weighing the residue, and the water by igniting with lead oxide and weighing the evolved water in a calcium chloride tube. Since half the available chlorine is the actual chlorine combined with oxygen, the total chlorine less one half the available chlorine is the chlorine not combined with oxygen and the total CaO, less the free CaO, is the CaO combined with chlorine, all the needed values are obtained from the above data. Six examples of the bleaching powder were analyzed and the results show that it contains the elements of calcium hypochlorite and calcium chloride in equal molecular proportions, but that the amount of calcium hydrate is variable, contrary to the conclusion of Stahlschmidt. The calcium chloride present in the bleaching powder was extracted by alcohol, and the total lime, the free lime, and the total and available chlorine determined in the residue. The results show that in the bleaching compound the lime is to the total chlorine as 1 : 2, to the actual oxidizing chlorine as 1 : 1 and the actual oxidizing chlorine is to the total chlorine as 1 : 2; all of which conditions are fulfilled and fulfilled only by the formula of Odling. The author therefore concludes: (1) that the excess of calcium hydrate present in bleaching powder is not a constant quantity; (2) that the formula of the bleaching compound is Cl–Ca–OCl; and (3) that by the action of water this compound undergoes decomposition as follows: $(Cl–Ca–OCl)_2 = Ca(OCl)_2 + CaCl_2$.—*J. Chem. Soc.*, xliii, 410, Oct., 1883. G. F. B.

2. *On the presence of Sulphurous oxide in the air of Lille.*—Ladureau has examined the atmosphere in Lille with a view to determine the presence of sulphurous oxide in it. His attention was first called to the matter by noticing that blue litmus paper became red when exposed to the outer air, and by the peculiar characteristic after-taste sometimes observed, on breathing it. Accordingly an aspirator of ten liters capacity was installed in a garden in the interior of the city, the air being drawn through two U tubes containing pumice moistened with pure sulphuric acid, and fragments of pure potassium hydrate, placed successively, then through Liebig's potash bulbs. The experiment continued for several months, many hundred cubic meters of air having passed through the apparatus. The potassium sulphites and sulphides were then oxidized to sulphates and the sulphuric acid determined as barium sulphate. As a result it appeared that each hectoliter of the air which passed through the apparatus contained 0·18 cubic centimeters of sulphurous oxide or 1·8 c. c. per cubic meter. The experiment was repeated, taking a time when the air was quiet, and the result showed 2·2 c. c. of sulphurous oxide to the cubic meter. On the other hand, the amount fell to 1·4 c. c. when the weather was windy. On examining the rain water of

the place, carefully collected, the sulphur products corresponded to a content of sulphuric acid of 0·022 grams per liter or 2·2 grams per hectoliter.—*Ann. Chem. Phys.*, V, xxix, 427, July, 1883.

G. F. B.

3. *On the atomic weight of Antimony.*—BONGARTZ has made a series of determinations of the atomic weight of antimony using for the purpose a method proposed by Classen which consists in oxidizing by means of hydrogen peroxide, the hydrogen sulphide set free from antimonous sulphide by hydrogen chloride. Pure antimony chloride, fractionated six or eight times, was digested with ammonium sulphide in excess in a platinum basin and the metal precipitated by means of electrolysis with a Siemens magneto-electric machine. It was obtained in brilliant plates from which the last traces of sulphur were removed by fusion with sodium carbonate and subsequent washing with dilute hydrogen chloride. The finely divided metal was heated with a concentrated solution of potassium sulphide, the solution was saturated with hydrogen sulphide, and precipitated with sulphuric acid. The washed antimonious sulphide was treated as described by Classen, i. e., placed in a flask with dilute hydrochloric acid, and the escaping hydrogen sulphide passed through a vertical tube filled with glass beads over which an ammoniacal solution of hydrogen peroxide trickled. The last traces of H_2S were washed out by a current of CO_2, the absorption tube was rinsed with distilled water, the solution acidulated and the sulphuric acid determined as barium sulphate. The results of twelve experiments are as follows: 120·170, 120·157, 120·091, 120·106, 120·114, 120·175, 120·390, 120·305, 120·310, 120·155, 120·206, 120·139; the mean of all being 120·193. This atomic weight confirms the values heretofore obtained by Schneider and by Cooke.—*Ber. Berl. Chem. Ges.*, xvi, 1942, Sept., 1883. G. F. B.

4. *On the constitution of Galician Petroleum.* — LACHOWICZ has submitted to examination the petroleum from Boryslaw in Galicia, using for the purpose first the commercial fraction having a boiling point from 30° to 125°. This was separated into three portions, of which the first boiled below 50°, the second between 50° and 80° and the third between 80° and 110°. From the first, isopentane boiling at 29°–30°, and normal pentane, boiling at 37°, were obtained. From the second, a normal hexane, boiling at 60° to 61°, and a secondary hexane boiling at 70° were obtained; and from the third a heptane boiling at 98·2° to 99·4°, was separated. In examining higher commercial fractions for hydrocarbons of the marsh gas series, nonane, boiling at 147·5° to 148·5°, and decane, at 152° to 153° were separated. Careful examination failed to discover in Galician petroleum any trace of hydrocarbons of the ethylene series. Two fractions, one boiling from 30° to 125°, mentioned above, the other from 20° to 110°, as also the crude petroleum, were examined for hydrocarbons of the aromatic series, and yielded benzene, toluene, isoxylene and mesitylene. The hydrogenized aromatic hydrocarbons found by

Beilstein and Kurbatow in Caucasian petroleum, though probably present as shown by the specific gravity, were not isolated. The fraction of Caucasian petroleum boiling between 95° and 100° has a specific gravity of 0·748, while the similar Galician fraction has a specific gravity of 0·7291 and that from American petroleum one of 0·7102.—*Liebig Ann.*, ccxx, 188, Aug., 1883. G. F. B.

5. *On the preparation of Mesitylene.*—VARENNE has made a series of experiments on the preparation of mesitylene based on the fact that this hydrocarbon results from a condensation of three molecules of acetone with the elimination of three molecules of water. The process which he prefers is as follows: The acetone (3 molecules or 180 grams) is mixed with 300 grams of sulphuric acid and allowed to stand for an hour. Then it is distilled over a naked fire, the heat being made uniform over the lower surface of the vessel. Toward the end when the mass swells up, a current of steam is passed into the flask, the fire is extinguished, and the operation stopped when no more striæ of mesitylene are seen on the condenser tube. The yield is satisfactory, the 180 grams of acetone giving 40 grams of crude product. It is purified by washing with sodium carbonate, by solution in ether and by distillation from calcium chloride.—*Bull. Soc. Ch.*, II, xl, 266, Oct., 1883. G. F. B.

6. *On certain derivatives of Benzil.*—BURTON has studied a compound obtained by Zinin by acting with hydrogen cyanide upon an alcoholic solution of benzil. It is an addition product of the formula $C_{14}H_{10}O_2(CHN)_2$ and was regarded as the nitrile of diphenyltartaric acid. When saponified, it would obviously yield an acid. After several attempts, the finely pulverized nitrile was placed in a large excess of glacial acetic acid saturated with HBr at 0°, and allowed to stand for some weeks with frequent agitation. From the solution, fine brilliant monoclinic crystals separated. On adding ammonium carbonate to the mother liquor, a white precipitate was thrown down, which after crystallization from alcohol, had the formula

$$C_6H_5C(OH)CO.NH_2$$
$$C_6H_5C(OH)CO.NH_2$$

On account of its becoming viscous no exact determination of the fusing point could be made. It softens at 150°, but is not fully melted until the temperature reaches 230°. It is soluble in hot water and in alcohol but not in ether. From its amide character it is only weakly basic. It dissolves in hot HCl and separates unchanged on cooling. It unites with HBr to form the beautifully crystallized compound just mentioned. This compound fuses at 185°, evolving HBr. Alkali carbonates convert it into the amide again. The amide has no acid properties and is insoluble in alkali carbonates and hydrates. The solution in sodium hydrate becomes deep red on warming, and on addition of an acid, deposits red flocks containing nitrogen.—*Ber. Berl. Chem. Ges.*, xvi, 2232, Sept., 1883. G. F. B.

7. *Relation between the Sun's spots and the Earth's temperature.*—At a meeting of the Physical Society in Berlin, Oct. 19, Dr. Frölich made a report upon the measurements of solar heat undertaken by him. The object of his work was to discover if the change in frequency of the sun's spots was accompanied by any change in the temperature of the earth. The instrument used by Dr. Frolich consisted of a thermo-electric pile inclosed in a wide double walled pipe, opening in front in the form of a funnel, in which circulated a constant stream of water at atmospheric pressure. The exposed front of the pile was closed by a plate of rock salt. The apparatus was capable of revolving in all directions. This apparatus was preferred to Langley's bolometer, since the electrical resistance of thin plates is subject to considerable variation during long periods of time. As a standard of heat the author employed a hollow screen filled with steam, one side of which was blackened with smoke and the other whitened with chalk. Measurements were taken on perfectly clear days, with the sun at different altitudes, and were represented by curves, the abscissæ showing the thickness of the atmosphere, and the ordinates the warmth of the sun. The measurements in the neighborhood of Berlin gave, in general, a straight line. One single measurement made upon the Faulhorn at a height of 9000 feet gave a perfectly straight line. Measurements distributed over the months of June, July, August, September and October, were different for each month. These measurements were compared with the daily photographs of the sun taken by Dr. Lohse at the Potsdam Observatory, and it was found that the lower degrees of solar heat corresponded with numerous formations of spots while the higher gradations of heat were accompanied by fewer spots. More observations, however, are necessary in order to prove this conclusively.—*Nature*, Nov. 8, 1883, p. 48. J. T.

8. *Theory of the Dynamo-Electric machine.*—Clausius enters upon a theoretical discussion of this machine which is much needed at the present time. The article is too long and mathematical for a suitable abstract. The question of the working of every machine as a magneto-electric engine first and afterwards, as the strength of the current increases, as a dynamo-electric engine is touched upon. The conditions of the revolving and non-revolving core of the armature are also examined.—*Ann. der Physik und Chemie*, 1883, No. 11, pp. 353–372. J. T.

9. *Aperiodic Galvanometer.*—M. G. L. Goarant de Tromelin describes a galvanometer which has three magnetic needles. The two lower ones constitute the ordinary astatic combination. The additional needle is placed above the astatic combination with its poles opposite to that of the needles immediately below it. The author states that this combination is nearly three times as sensitive as that with the astatic combination alone. In another arrangement the combination consists of horse-shoe magnets placed horizontally one above the other at a distance of $0^{m}{\cdot}005$ and fixed in position. The movable part of the apparatus con-

sists of a small coil of wire surrounding the poles of the central magnet and suspended by a bifilar arrangement which conducts the current to the coil; the arrangement being similar to that adopted by Sir William Thomson in his syphon recorder. This apparatus is perfectly aperiodic and is so sensitive that a few milligrams of iron filings falling upon the pole of a magnet of a telephone—the vibrating plate having been removed—give a sensible deflection.—*Comptes Rendus*, Nov. 5, 1883, p. 995. J. T.

10. *On the Heat Produced in Iron and Steel by Reversals of Magnetization;* by JOHN TROWBRIDGE and WALTER N. HILL.—The object of our investigation was to determine whether the heat which is generally attributed to rapid magnetizing and demagnetizing is really due to this cause, or to induction currents in the mass of the iron. Our work, however, had in the beginning a more practical object. Since cylinders of iron and steel are heated when they are made the cores of electro-magnets, and are submitted to the effects of rapidly alternating currents, it was thought that they might exhibit different degrees of heating, and therefore that a process of determining the character of iron and steel might be based upon the phenomena observed, which could be called an electro-magnetic criterion of certain physical properties of these metals. It is well known that chemical analyses of steel and iron throw very little light upon their physical properties, such as tenacity and elasticity in general. There is no satisfactory test for the properties of different steels save by a testing machine, and this is not readily applicable in many cases. If a method could be devised which depended simply upon electrical and magnetic phenomena, it would be a valuable aid to the metallurgist.

The amount of sulphur, of phosphorus, and other ingredients besides iron, is very small in steel, and it could hardly be expected that their presence or absence could be detected by the difference of heat developed under the influence of alternating currents, unless this heating is really due to molecular agitation produced by magnetization and demagnetization. If the heating is due to alternating induction currents in the mass of metal, there should be very little difference in the amount of heat developed by different specimens of steel; for their electrical resistance would not differ sensibly from the presence or absence of a fraction of one per cent. of phosphorus or sulphur. If the heating is due to magnetization or demagnetization, and to an actual twisting of small magnets in their beds, then the molecular arrangements consequent upon different admixtures of various ingredients might produce more heat in one specimen than in another, and thus afford a criterion of the character of the steel. * * * *

[The description given of the experiments is here omitted.]

These results show that this method affords no criterion of the physical properties of iron and steel. The molecular structure of the various specimens employed was not sufficiently modified to enable us to determine any differences in molecular heating,—if

the heat developed by magnetizing and demagnetizing is due to molecular heating. If it is due entirely to induction currents in the metals, the slight changes in electrical resistance produced by small quantities of sulphur, of phosphorus, and of carbon would be inappreciable in the masses of iron which we used, and we should expect to obtain under the same conditions the same rise in temperature for the different specimens of steel. Our previous work* on cobalt and nickel must therefore have been affected by some error.

We next determined to ascertain if the heating was confined to the surface of the metallic cores. Theory indicates this to be the case, whether we adopt the hypothesis that the heat is due to magnetization and demagnetization, or the hypothesis that it is produced by induction currents. We could not find, however, any experiments upon this point. The bars were prepared as follows: Each one was bored one-half its length. At the outer end of the hole a shoulder was turned in order that a short piece of glass tubing could be cemented in. One thermometer was placed in the mercury surrounding the bar of iron, and another was hung in the hole in the center of the bar, the hole being also filled with mercury. It was difficult to distinguish between the conduction of heat and the evolution of heat. The rise of temperature indicated by the inner thermometer, however, was probably entirely due to conduction of heat, as can be seen by comparing the amounts of mercury surrounding the two thermometers.

If the heating is due to molecular movements produced by magnetizing and demagnetizing,—and the musical note is adduced as an evidence of this,—the bar would vibrate as a whole, and would become heated throughout on account of this vibration. It is difficult to conceive how the surface action of magnetism can communicate vibrations to a solid bar of iron one inch and a half in diameter. If the bar vibrates as a whole, a certain amount of heating of the bar takes place throughout its interior. The heat in the interior of the bar, however, must be less than that at the exterior, where the magnetization exists in full strength. We believe, however, that the musical note is due to a forced vibration in the coil of the electro-magnet,—possibly due to electro-magnetic attractions; for the note can be heard when the iron core is removed, and is stronger when the core is in place simply because the magnetic field is strengthened.

The appearance presented by iron filings strewn upon the pole of a straight electro-magnet, which is submitted to the action of an alternating current, shows very strikingly the fact that it takes time to magnetize, and that magnetism resides upon the exterior of electro-magnets. Under the influence of strong currents alternating six thousand times a minute, the electro-magnet is still capable of attracting an armature with great force. The filings arrange themselves as a narrow fringe or ring upon the circumference of the end of the cylindrical bar constituting the

* Proceedings of the American Academy, 1878–79, p. 114.

core of the electro-magnet, leaving the surface of the end of the cylinder entirely free from filings. If filings are scattered upon this free portion of the surface, they waltz to the circumference. The fringe of iron filings vibrates in unison with the alternating currents. In connection with this investigation, it may be interesting to refer to some experiments made by Lt.-Comm. A. G. Caldwell, U. S. N., and ourselves, on demagnetization. These experiments were made in 1880, but have not been published.

Perfect demagnetization, or entire absence of magnetism in a mass capable of magnetism, is a condition of great rarity. Approximate demagnetization has been brought about with some difficulty, but delicate tests would show traces of polarity.

We have, however, discovered a method by which complete demagnetization may be rapidly and easily produced. The principle involved is the setting up of a state of powerful magnetic vibration, by which all previous magnetic conditions are obliterated, and on the subsidence of which no polarity remains. This state of vibration is induced by an alternating current of *sufficient strength*. By this an effect is induced in the magnetized mass which can only be compared to a vibration or wave. The reversals of the inducing current cause corresponding reversals of polarity in the body acted on, and as these reversals are continuous and very rapid (5,000 to 6,000 a minute, for example), a molecular vibration probably arises. It is probable that a condition of strain or set is one of the phenomena of magnetism.

The particles have been made to assume a certain definite or polar relation or position. When, however, a powerful movement or vibration is caused, it is evident that when this vibration has become complete,—that is, involving the whole mass,—all previous conditions of strain or "permanent set" will be overcome. It must be remarked that, in order to perfectly attain this result in all cases, the exciting force must be sufficient.

When the alternating current ceases, the body acted on is left perfectly free from polarity. It is, however, in a state of extreme sensitiveness, and must be allowed to remain at rest for a short time. If it is placed north and south, it will assume polarity, and very strongly if struck with a hammer when held in the position of the dip.

Demagnetization requires but a short time in most cases,—from one to three minutes if the current is properly adjusted. There are several ways of performing the experiment, but it will be sufficient at this time to refer to a few of them. The most effective method is to enclose the mass to be demagnetized in a coil of such a length that the whole body will occupy an approximately central position. The coil may be a simple one, in which case it must stand east and west, and before removing the object the electric machine must be stopped, and the current allowed to die away. Also, when the object is taken out of the coil, it must be carefully shielded from the earth's induction; or the coil may be so constructed that it can be opened or divided at the center

without breaking circuit, and then the object can be taken out without stopping the alternating current. One of the coils we used was made of No. 12 copper wire, wound as one coil, but in halves, with an elastic connection. It is well known that, with an alternating current, self-induction in the coil materially reduces the current, and therefore the coil should be one of a comparatively small number of turns.

Demagnetization of small masses, not too retentive of magnetism, may be performed by placing them on the end of a bar contained in the coil, which is a part of the alternating circuit. A bar of low steel, somewhat longer than the coil, was used, and the small objects placed on its projecting end.

Perfect demagnetization is attained with varying difficulty. Ordinarily, it is rapidly and easily accomplished. Sometimes a longer time is required, or a more intense action. In numerous experiments we derived the alternating current from a Wilde machine which gave about 6,000 reversals per minute. This rate is probably greater than is required or desirable, except in extreme cases. About 3,000 or 4,000 reversals would be a better general rate, although of course the operator should arrange his apparatus so that he could get more reversals if necessary. Failure will result if the speed is too great, as might be expected if the view here taken is correct. Usually it will be more convenient to employ, instead of an alternating machine, a battery with a reverser arranged for varying speeds.

One application to this method is to the demagnetization of watches. Watches strongly magnetized are completely demagnetized by one to three minutes' exposure in the coil. Frequently unsuspected traces of magnetism cause annoying irregularity of action of a watch. This method enables us entirely to remove this difficulty.

Some very curious and interesting results were obtained by experimenting with magnetite.

A specimen of very pure magnetite from North Carolina, showing marked magnetic properties, was completely demagnetized by a somewhat long exposure to the action of an alternating current applied as described above. Before demagnetization the piece had shown consequent points, although in general it possessed polarity. After treatment it attracted either end of a very light suspended needle indifferently, and when any part of the mineral was presented, just as a piece of soft iron would do. The demagnetized specimen was then placed across the poles of an electromagnet excited by a strong current from a Gramme machine. It became strongly magnetic, with distinct poles, and without the consequent points it at first had. After this it was treated like an ordinary bar magnet, and magnetized or demagnetized at will.

Another more impure piece was originally less strongly magnetic, and was demagnetized with great difficulty. At first it displayed no general polarity, having consequent points irregularly distributed. After demagnetization it received induced magnetism

and became polar, but it was a much feebler magnet than the previous specimen. Demagnetization was afterward performed more easily than at first.

A still more impure specimen was treated, but with the means at hand it was not *perfectly* demagnetized, although so nearly was this done that only traces of magnetism were noticeable.

The result of our work can be stated as follows:

(1) The heat developed by reversals of magnetization is probably due to induction currents, and not to molecular vibrations; for considerable changes in the molecular structure of different specimens of iron and steel fail to show differences in the amount of heat developed.

(2) The heating of iron cores o electro-magnets, which are submitted to alternating currents, is confined to the surface until conduction equalizes the heat of the cores.

(3) The musical note emitted by the core is the note of the coil, due to the number of reversals of the machine, and is merely strengthened by the metallic core of the electro-magnet. This note should not, therefore, be used as an argument in favor of molecular vibrations of magnetic particles.

(4) Experiments on demagnetization confirm what has long been known in regard to the effect of vibrations and shocks upon the magnetic condition of iron and steel. They do not invalidate our results upon the heat produced by reversals of magnetization; for a very slight change in position of the molecules might affect the magnetism of a bar, and yet be insufficient to produce the great heating observed in the armatures of dynamo-electric machines.—*Proceedings American Acad. Arts and Sciences, May* 29, 1883.

11. *On the properties of water and ice;* by OTTO PETTERSSON.—The above is the title of a valuable paper by Dr. Pettersson taken from volume II of the results of the Vega Expedition. It is devoted to a discussion of the physical properties of water in the liquid and solid state at the temperatures to which water and ice are exposed in the Arctic Sea, as between $-20°$ C. and $+15°$ C. The special subjects investigated were the change of heat and volume of (1) pure water, (2) brackish water, and (3) ocean water of ordinary saltness, but before detailing the results of his experiments the author states briefly the results reached by others, and then in detail the instruments employed by him and his methods of observation. In regard to the experiments with pure water, a series of tables gives the results of the experiments on the expansibility of pure water (ice) at the different temperatures employed. The most important point brought out in them is this: that the volume of ice, even the purest which can be tested, decreases with a rise in temperature when near the melting point and this is the more marked as the amount of salt contained in the water increases. In regard to this the author remarks that, "It is impossible to decide if absolutely pure water would be entirely free from this weakness or not, since we cannot assume that water, which has boiled for a quarter of an hour or more in a

glass vessel, is absolutely free from minimal quantities of foreign substances, as e. g. sodium salts, silica, etc. For my own part I am rather inclined to think that absolutely pure water, if it could be tested, would show an absolutely fixed melting point . . . ." In connection with this point the author remarks upon the plasticity of ice, which has been observed near the melting point, while at lower temperature it is brittle. The author shows that the irregularity in expansion is connected with the degree of rigidity of the ice: at a sufficiently low temperature every kind of ice is hard and brittle, and in this state it expands regularly with increase of temperature; the commencement of the thawing is entirely dependent upon the amount of salt, etc., present, and the softening before melting is connected finally with a contraction in volume: thus ice from ordinary distilled water begins to contract at $-0{\cdot}25°$ C.; a sample containing 0·015 p. c. chlorine at $-4°$ C.; and ice formed by the sudden freezing of ocean water begins to contract at $-20°$ C. or below. As a consequence of this it is added that it must be acknowledged "that the ice masses of the glaciers are liable to contraction at temperatures below their melting point."

In the investigation of the chemical changes in the composition of sea water caused by freezing, the author reaches the conclusion that ice formed from sea-water is not, as has generally been supposed, essentially pure, owing its saltness to mechanically enclosed brine; the result reached is the same as that obtained in a different way by Dr. Buchanan of the Challenger. Dr. Pettersson concludes that ocean-water is divided by freezing, not into pure water and a more or less concentrated solution of ordinary sea salt, as was formerly believed, but into two saliniferous parts, one liquid and one solid, which are of different composition. Thus the formation of sea-ice is chemically a selective process, some of the elements of salt water are more fit than others to enter into the solid state by freezing, those which are rejected by the ice will preponderate in the brine, and vice versa; for example as regards the relation of $Cl : SO_3$, the ice is richer in sulphates, the brine in chlorides. The extraordinary variation both in saltness and in chemical composition of every individual specimen of sea ice and sea-brine depends upon a secondary process or the metamorphosis of the ice, due to the combined influences of time and variation of temperature. The concluding chapter is devoted to a discussion of the latent heat of fresh and salt water; the important point brought out in it is this, that the latent heat developed by the freezing of sea-water is "extraordinarily inferior to that of pure water." The author adds, that the freezing process, however, from a thermic point of view, is not entirely concluded with the solidification of the sea-water, by which it is divided into ice, solid cryohydrates, and liquid brine containing dissolved salts; for on further sinking of the temperature, still unfrozen cryohydrates will be solidified and develop heat until the whole mass, at a sufficiently low temperature is a solid rock of crystal-

lized matter. At the rise of temperature these substances will melt one by one, and absorb heat in so doing.

12. *Contributions to the Hydrography of the Siberian Sea;* by OTTO PETTERSSON.—This is a second paper by the same author as that above noticed, it contains a long and valuable series of observations made in connection with the Vega expedition, of the temperature of the sea-water at different depths, specific gravity and the percentage of chlorine, as also that of salt deduced from the observed specific gravity. These observations were taken mostly in the Kara Sea, an admirable hydrographic chart of which accompanies the memoir.

II. GEOLOGY AND MINERALOGY.

1. *Second Annual Report of the U. S. Geological Survey to the Secretary of the Interior, for the year* 1880–81; by J. W. POWELL, Director. Washington, 1882; pp. iv and 588, 4to, with LXI Plates, Figs. 1–32, and a Map, in pocket, showing distribution of the strata and eruptive rocks in the Western part of the Plateau Province.—This volume is the first annual report of the Geological Survey issued under the directorship of Mr. Powell, who, at the close of the fiscal year for which it is issued, had held the position for less than three and a half months. The work reported upon was begun under the direction of his predecessor, Mr. Clarence King, and no change was made in his plans and methods. The volume opens with the Report of the Director, which is followed by the administrative reports of the heads of divisions and by abstracts of the several monographs which are in preparation by the Survey. The monographs are especially designed for the use of specialists, but the summaries given here present the facts and conclusions in a manner adapted to the general reading public. These papers are as follows: The Physical Geology of the Grand Cañon District, by Capt. C. E. Dutton; Contributions to the History of Lake Bonneville, by G. K. Gilbert; Abstract of Report on Geology and Mining Industry of Leadville, Lake County, Colorado, by S. F. Emmons; A Summary of the Geology of the Comstock Lode and the Washoe District, by George F. Becker; Production of the Precious Metals of the United States, by Clarence King. The volume ends with a paper by Mr. G. K. Gilbert on "A New Method of Measuring Heights by Means of the Barometer." Both of Mr. Gilbert's papers, and also the one by Mr. Emmons, have already been noticed in this Journal, as have also two of the monographs which have been issued since the publication of this annual report, viz: those by Capt. Dutton and Mr. Becker.

The Report of the Director is introductory to the volume. It epitomizes the various papers included and details the plan of publication. Reference is made to a report, in preparation by Eliot Lord, on the history of the Comstock Lode, and an account is given of Dr. R. D. Irving's investigation of the copper-bearing

rocks of Lake Superior. A statement is also made of a few rules adopted by the Survey, in relation to general geologic nomenclature, cartography and diagrams. The scheme of geologic nomenclature, as laid down in Dana's Manual and subsequently adopted by LeConte in his Elements of Geology, will be followed without material change. In the opinion of the Director, it is unwise for the exploring geologist to commit himself in early stages of investigation to refined and exact correlations, and in the scheme adopted by the Survey the column of epochs or formations is left to be formulated in the various districts as the facts demand. A scheme of colors for geologic charts and of conventional characters for diagrams is presented with illustrations, and the report concludes with a financial statement for the fiscal year ending June 30th, 1881.

Among the administrative reports, that of Mr. Arnold Hague is of some length and considerable interest. It is accompanied by a double page geologic map (in colors) of Ruby Hill Eureka Mining District, Nevada. Mr. Hague reports on the field work in this district, which ranks so high as a mining center, that for economic purposes its elaborate study is important. Its general geological structure has been investigated, and is to be supplemented by a study of its mining geology. As the Director says in another place, mining geology is superficial and almost valueless unless it has a solid foundation in structural geology, and for this reason it is the policy of the Survey to conduct both lines of research in every special district that is taken up for examination. Mr. Hague's field work is completed, and he states that the main features of his monograph on the district will be as follows:

1st. A description of each mountain block, with a detailed account of the formations and their relations to each other.

2d. A detailed description of the great series of Paleozoic sediments, embracing 20,000 feet of strata, extending from the base of the Middle Cambrian up into the upper Coal-measure limestone.

3d. An account of some of the more important dynamical events that have produced the present physical features of the country.

4th. A history of Tertiary volcanic activity, during which all the principal types of volcanic rocks except trachyte found their way to the surface. The relation of the sedimentary beds to the volcanic rocks will also be discussed.

5th. A statement of the position and geologic horizon of the several mining districts and some of the more important mining properties, and some observations on the relations of the ore deposits to both Tertiary and Pre-Tertiary crystalline rocks.

Mr. Hague's monograph is to be accompanied by an atlas of twelve sheets, on which twenty-five colors will be required to represent the Paleozoic system of the district and nine colors will be needed for the igneous rocks. A special monograph on the microscopic petrography of the district by Mr. Joseph P. Iddings is to accompany it; also a report by Mr. Charles D. Walcott. The latter will undoubtedly be one of the most valuable contribu-

tions ever made to the paleontological history of the Great Basin. Heretofore this district has been considered a poor one for organic remains, but the present survey of it has secured a collection of nearly 5,000 specimens, which is probably the largest collection of Paleozoic fossils ever obtained from so limited an area in the far West. It is, however, not the number of specimens nor the number of species represented, which Mr. Walcott has determined to be 359 (more than a third of them new), that gives greatest value to this collection, but the fact that it is a systematic collection in one district, extending through one series of 20,000 feet of Paleozoic sediments, included between the base of the Middle Cambrian and the upper Coal-measure limestone. The Cambrian fauna embraces 119 species; the Devonian, 144, and the Carboniferous, 83. In the Silurian the fauna is but slightly developed, the material being derived mainly from the Trenton formation. Thirteen species which suffice to determine the horizon were found, but they are too fragmentary for detailed study.

In an administrative report by Prof. R. Pumpelly, statistics are given of the production of coal, copper, iron, lead and zinc. The collection of these statistics was made in connection with the Census Bureau, and they embody the information obtained from the canvass of 10,440 mining establishments.

The paper by Mr. Clarence King on the Production of the Precious Metals was also prepared in coöperation with the Tenth Census, and is preliminary to a monograph to be published by the Survey. The present paper is practically the same as the one on the same subject published by the Census Bureau, and which has already been noticed in this Journal.

The work is handsomely illustrated, especially in Capt. Dutton's paper, many of the plates and figures being made from sketches by Mr. W. H. Holmes, whose hand has lost none of its cunning.

A double column index of twenty pages effectively concludes this valuable report.

2. *Third Annual Report of the U. S. Geological Survey to the Secretary of the Interior*, 1881–82. By J. W. Powell, Director. Washington, 1883. 4to, pp. xviii and 564, with XXXV Plates and Figs. 1 to 56.—A few advance copies of this report have been issued without the complete set of illustrations. The volume opens with the Report of the Director, which gives a résumé of the work accomplished during the year and presents a financial statement. Following this are administrative reports by Messrs. Clarence King, Arnold Hague, G. K. Gilbert, T. C. Chamberlin, S. F. Emmons, G. F. Becker, L. F. Ward, J. Howard Gore and Gilbert Thompson.

Mr. Hague in his report states that he, in connection with Mr. J. P. Iddings, has undertaken an investigation of the acidic volcanic rocks collected by the geologists of the Geological Exploration of the 40th Parallel, including not only the rocks reported upon by Prof. Zirkel in his Microscopical Petrography, but also a large amount of material not prepared at the time his report

was published. The result of this investigation has been to correct a grave mistake in Prof. Zirkel's work where he frequently calls the feldspars in certain rocks, sanidin, when they are really determinable as belonging to triclinic species. Messrs. Hague and Iddings find that, in the area covered by the explorations of the 40th Parallel Survey, there are, so far as represented by the collections, no normal trachytes, and that the rocks which have been so classed are andesites. Mr. Hague says that this result is the more remarkable, as rhyolite is by far the most abundant of all the acidic volcanic rocks of the Great Basin. The study of these results leads them to make the broad general statement that among the products of volcanic action in the Great Basin (at least so far as the area indicated above is concerned), the occurrence of orthoclase rocks is dependent upon the percentage of silica in the rock. A preliminary examination has also been made of the rocks from the great cones of Mt. Rainier, Mt. Hood, Mt. Shasta and Lassen's Peak, and the announcement is made that they are andesitic volcanoes, not a single trachyte being included in the collections from the four volcanoes.

After the administrative reports, which space does not allow us to notice further, is a series of "Accompanying Papers" as follows: Birds with Teeth, by O. C. Marsh; the Copper-bearing Rocks of Lake Superior, by Roland D. Irving; Sketch of the Geological History of Lake Lahontan, by Israel C. Russell; Abstract of the Report on the Geology of the Eureka District, Nevada, by Arnold Hague; Preliminary paper on the Terminal Moraine of the second Glacial Epoch, by Thomas C. Chamberlin; and A Review of the Non-marine fossil Mollusca of North America, by C. A. White.

Prof. Marsh in his paper gives a résumé of his investigations of all the known forms of the oldest birds, which differ from recent birds, in having well developed teeth. Prof. Irving's paper, of 102 pages, is an abstract of Vol. V, of the Monographic Series of Reports of the Survey, which is in an advanced stage of preparation. Mr. Russell's paper is also an abstract of one of the monographs. The basin of Lake Lahontan, a Quarternary lake of Northwestern Nevada, according to Mr. Russell, exhibits a vast amount of calcareous tufa, of which there are three varieties, characterizing three distinct deposits in the basin. They are designated, in the order of their age, as *lithoid*, *thinolitic*, and *dendritic*. The first of these reaches the level of the lithoid terrace about 500 feet above the present level of Pyramid Lake and was the first formed. At the top it is only a few inches thick, but increases as the slopes are followed downward. The *thinolitic* tufa layer was deposited at a lower water-stage and reaches only 100 feet above the present lake-level. It differs from the other tufas of the basin and from calcareous tufas in general, in the fact that it is composed of crystals. It has a thickness of six to eight feet. The third and most abundant of all the chemically formed rocks of the basin, is the dendritic tufa, which is locally twenty feet and possibly fifty feet thick, and extends about 300 feet

above the present lake-level. These deposits show at least three well-defined periods in the history of Lake Lahontan. When the first was formed, the lake, then a fresh-water body, as shown by the fossils, filled the basin to within thirty feet of the highest water-line now scoring its sides. Later the water was 400 feet lower and it was highly charged with soda. Then the thinolite terrace was carved. Later still the water rose 200 to 250 feet above this terrace and the heaviest tufa deposit was precipitated. There were, therefore, two moist periods, when the lake was deep, separated by a time of dessication and followed by the present period of aridity.

Mr. Hague, in his abstract on the geology of the Eureka District, gives the Eureka section a thickness of 30,000 feet, divided as follows: Cambrian, 7,700 feet; Silurian, 5,000 feet; Devonian, 8,000 feet, and Carboniferous 9,300 feet. The mingling of Coal-Measure and Lower Carboniferous types is more clearly shown in the Carboniferous rocks of the Eureka Basin than in any other western locality.

In the paper on the terminal moraine of the second glacial epoch, Prof. Chamberlin not only abstracts the results of recent investigations by himself and assistants, but combines therewith the observations of others; he gives a description of a compound moraine traced continuously from the Atlantic coast in New England, through New Jersey, Pennsylvania, Ohio, Indiana, Illinois, Wisconsin, Minnesota, Iowa, and finally Dakota, where, at the 103d meridian, it passes into British America. His contribution to glacial geology is too important to be thus briefly characterized, but space will not permit of more at present.

Dr. C. A. White's paper on the non-marine fossil Mollusca of North America, which concludes the volume, is one of the most interesting in it, especially to those who have followed the controversy as to the age of the Laramie group. The paper is accompanied by thirty-two plates, one of which is devoted to Devonian forms, one to Jurassic and Triassic, two to Cretaceous, three to the Bear River Laramie, and the remainder to the Laramie group proper. These plates illustrate an annotated catalogue which is a résumé of the subject by zoological families. A tabular view is also presented. There is besides, an introduction which states the object and method of the work, and a general discussion which ends the paper. The space at command here will not permit of reference to more than one or two points. Three categories of non-marine mollusks are embraced by those of brackish-water, those of fresh water, and those of land habitat. Dr. White points out the analogy between the ancient Laramie Sea and the Black Sea, which in his opinion, the Laramie hydrographic system more nearly resembled. He also states the conclusion that the molluscan fauna of the present Mississippi system is lineally descended from the faunæ of the ancient lakes and seas of the Tertiary and Laramie periods and the river systems of which they constituted lacustrine portions. Dr. White is the first to emphasize this fact.

Heretofore the commingling of brackish-water and fresh-water fossils has been explained on the supposition that the beds in which they were found were estuary deposits. Dr. White says that there is evidence "that the fresh-water fauna proper of the Laramie system not only inhabited the streams which emptied into its sea, but that in great and shifting areas of the sea itself, the waters were sufficiently fresh to allow the existence in them of such Mollusca as *Unio*, *Goniobasis*, *Viviparus*, *Campeloma*, etc., and saline enough in other parts for the existence of *Ostrea*, *Anomia*, *Corbula*, etc." The volume is handsomely illustrated and has a full table of contents and index.

3. *Geology of Lehigh and Northampton Counties, making Report of Progress D3, Vol. I, of the Second Geological Survey of Pennsylvania.* 284 pp. 8vo, with illustrations in the text and a thick atlas in 8vo of over 30 folded sheets. Harrisburg, 1883. —This volume contains the following reports: on the slate-belt and quarries, by R. N. SANDERS; water-gaps, by H. M. CHANCE; on the limestone belt and the Potsdam rocks, by F. PRIME; an itinerary survey of the South Mountain gneiss, etc., by C. E. HALL; and a "Prefatory Letter" and "General Introduction," reviewing many points in the geology of eastern Pennsylvania, by Professor Lesley, the director of the survey.

Mr. Sanders gives, in addition to his descriptions of the slate-quarries, several figures illustrating well the transverse relations of the cleavage and bedding. The dip of the cleavage planes is stated to be in general parallel to that of the axial plane of the flexures in the stratification; the strike is usually the same for bedding and slates; and the exceptions in general occur where the beds bend around in the head of the flexure.

Mr. H. M. Chance describes and figures sections of the slate-belt at the Schuylkill, Lehigh and Delaware water-gaps.

Mr. Prime's report is on the limestone region of Northampton County—the county which lies against the middle of the eastern boundary of the State, and on the Potsdam sandstone in the same region. It covers a little over fifty pages of the volume.*

The limestone is the metamorphic limestone of the Great or Kittatiny valley, which extends southwestward from Northampton County into Lehigh and Berks counties, and beyond. In a former report (DD) the author proved, by the discovery of fossils, that this limestone in Lehigh County (the next county to the southwest) was Lower Silurian, of the age of the Chazy and Trenton. In this volume he announces the discovery in it of similar fossils in Northampton County. The following are some of the facts: 1¼ m. S.W. of Bath, occur species of the genus *Maclurea* or *Euomphalus* (not determinable, because poorly preserved) which are probably of Chazy age; a short distance N.E. of this locality and also at Christian Springs are *Crinoidal stems*, which are abundant and of Trenton age; just outside of Nazareth,

* For some unexplained reason, Mr. Prime's name is in the running title of the whole volume.

besides crinoidal remains, a few specimens of *Orthis testudinaria;* nearly ¼ m. S.W. of Stockertown, similar *crinoidal stems, Chætetes lycoperdon* and *Orthis testudinaria;* at Churchville, along with the same crinoidal stems, *Leptæna sericea, Orthis testudinaria, O. pectinella;* on the Delaware, south of and close to Howell's cotton-mill, besides the species of *Leptæna* and *Orthis* just mentioned, also *Strophomena alternata* and *Chætetes lycoperdon*, the specimens in some spots crowded. Mr. Prime concludes from this best of evidence that the limestone formation is in part *Trenton*, in part *Chazy*, and infers doubtingly, on lithological evidence, that the limestone on the north flank of the South Mountain range is *Calciferous;* and since the Trenton limestone—in many places holding its characteristic fossils—lies conformably beneath the adjoining slates and shales, these slates and shales must be of *Utica* and *Hudson River* age; and he adds "there is not a particle of evidence that any of these limestones belong to Huronian or older periods." At one locality graphite scales occur, with traces of brachiopod shells. The limestone has in some parts intercalated layers of hydromica slate, and others of quartzyte. These and various other points connected with the limestone formation are discussed at length in the report. The facts as to fossils, geological age, metamorphic character of the limestone, and association conformably with hydromica slates, are precisely in accord with those connected with the great metamorphic limestone on the west part of the Green Mountain region in Vermont, where the slates are those of the Taconic range.

Professor Lesley reviews the topography of Northampton County and the associated region to the east, south and southwest; describes the sink-holes, and the underground drainage connected therewith, in the limestone region; and states in brief the geological constitution of the ridges and valleys. The question as to unconformability in Pennsylvania between the Lower Silurian and the overlying beds—the Oneida Conglomerate—is decided by him adversely. Professor Lesley continues with a discussion of the general question whether unconformability—as a result of an epoch of mountain-making—exists in the region of the Hudson River valley to the north, where such evidence has been thought to indicate a disturbance after the Lower Silurian era. He commences by saying that out of the alleged facts "an hypothesis has been framed and made to apply to a thousand miles of the continent;" but by whom, he does not state, or in what publication; the published conclusions, as far as the writer knows, confining the supposed upturning to the regions east of the Hudson, with a narrow strip west of part of it, and the extension of the unconformability southwestward being treated simply as a proper subject for investigation. His arguments against the unconformability in the Hudson River valley are based on general considerations, not on facts derived from personal observations in the region, such as seem necessary to settle a question in stratification. The fact that there is a great gap in the series of forma-

tions above the Lower Silurian, including all of the Oneida, Medina, Clinton and Niagara formations, except a few yards of the latter, which all New York geological observers have described, is set aside by arguing the case. Mr. Davis's statement with regard to the conformability of Lower and Upper Silurian strata at the eastern base of the Catskills, that gap in the series excepted, is cited as in opposition; while in fact the region is outside (west) of the supposed area of upturning. Other arguments are based on facts derived from localities "far south of the Hudson River belts" in New Jersey and Pennsylvania, which also are outside of the area of Green Mountain disturbance, and affect only the question that future geologists will discuss with new facts—as to the southwestern extension or limit of the Green Mountain disturbance. The facts in Mr. Davis's excellent papers on Becraft's Mountain and the Rondout region, published in this Journal since Professor Lesley's report appeared, are to the point, and of the kind needed to settle the question. His observations about Becraft's Mountain put the question in just the state in which it was left, in the writer's hearing, by the New York geologists, at a meeting of the American Association of Geologists and Naturalists over thirty-five years since; they afford some reason for doubting the unconformability, but stronger for sustaining it. The Rondout facts are of the positive kind, too positive to be disturbed by imagined or not proven overturns. The facts favor strongly the view that the close of the Lower Silurian was an epoch of mountain-making, along the Green Mountain region. That an epoch of some degree of disturbance in Eastern North America intervened directly after the Lower Silurian—is shown also by the Lower Silurian uplift about the Cincinnati region, well proved by Newberry, and that on the same southwest and northeast line (parallel to the general course of the Appalachians) in Tennessee, made known by Safford's investigations.

Professor Lesley reviews also facts connected with the stratification of the limestone, the Archæan rocks of South Mountain, and the Ice-age. J. D. D.

4. *Pennsylvania Geological Survey.*—Besides the volume above noticed, the survey has recently issued the following reports. No. AA, of plates, The Southern Anthracite Field, vol. I.—No. AC, with an atlas, On the Mining Methods and Appliances used in the Anthracite Coal Fields, by H. M. Chance.—No. C4, The Geology of Chester County after the surveys of H. D. Rogers, P. Frazer and C. E. Hall, edited by J. P. Lesley.—No. D3, Berks County, by E. V. D'Invilliers.—No. D5, Adams, Franklin and Cumberland, maps.—No. I4, On Warren County and the neighboring Oil Regions, by John F. Carll.

5. *The Contents of a Bone Cave in the Island of Anguilla;* by E. D. Cope. Smithsonian Contributions to Knowledge, No. 489. Washington, 1883. 30 pp. 4to, with 5 plates.—This paper is a full description of the bones from the Anguilla Cave—partially described by Professor Cope in 1868. The extinct species

are large Rodents of the genus *Amblyrhiza* and a small Ruminant, probably of the Bovidæ, besides others not determinable; and in the same cave were obtained a scraper or chisel made by man from the large shell *Strombus gigas*. Whether the Amblyrhiza and the Ruminant are of the same era, or man and either of these species, is not ascertained. Professor Cope remarks that the deposit containing the Amblyrhiza is not in his view "earlier than the Pliocene." The paper closes with the following inference. "The island of Anguilla, now embracing but 30 square miles could not readily have supported a fauna of which these huge Rodents formed a part." "This, and other facts mentioned by Pomel, lend probability to the hypothesis of the latter author, that the submergence of the ranges connecting many of the islands of the Antilles has taken place subsequent to Pliocene times." Such facts bear also on the question of a coral reef subsidence in those seas since the Pliocene.

6. *On the Chrysolitic beds or Dunyte of North Carolina;* by A. A. JULIEN. (Proc. Bost. Soc. Nat. Hist., xxii, 141, December, 1882.)—In this important paper, Professor Julien shows that the well known dunyte of North Carolina, which occupies "mainly a zone in the mountain plateau between the Blue Ridge and the Great Smoky Range 250 km. long and 15–30 km. wide" is distinctly bedded, and is enclosed in a stratum of black and slaty hornblende-gneiss, in a region of gneisses and other schists. The rock has a slaty lamination due to bedding, like that of the hornblende gneisses, and it is conformable to the latter in strike and dip with only local exceptions. At a few localities it is interbedded with the latter in layers 1 to 6 yards thick. Professor Julien thus proves, as he urges, that the chrysolite rock was originally of sedimentary origin like the associated schists. He also treats of the alterations of the dunyte, under the heads of Chalcedonic, Hornblendic, Talcose, Ophiolitic and Dioritic. All the hornblende within the chrysolite rock is attributed to alteration, like the serpentine, talc, chalcedony, and this rock is supposed to have originated in sedimentary deposits of chrysolite sand. But the occurrence of the chrysolite stratum in and among hornblende schists suggests that the hornblende of these schists and of the chrysolite was alike in metamorphic origin and source, and it seems to be hardly probable that the material so changed was throughout chrysolite.

7. "*Lenticular Hills.*"—Note by Prof. C. H. HITCHCOCK. (Letter to J. D. Dana, dated Hanover, N. H., Nov. 27th.)—It is not common for me to find some of my views or descriptions ascribed to my father; but in your remarks about the "lenticular hills" (this Journal, III, xxvi, p. 358), you speak of them as having been described by him. I am glad to say that he would have invented a better expression than "lenticular" hills, if he had described them. I suppose we may call them "drumlins," as that term has been applied to them in Scotland.

8. *On the probable occurrence of Herderite in Maine;* by WM. EARL HIDDEN. (Communicated in a letter dated Newark, New Jersey, December 11, 1883.)—To Mr. Nathan H. Perry, of South Paris, Maine, an earnest and successful collector of Maine minerals, and the discoverer of many new mineral localities in his State, I am indebted for a few specimens of a dark, oil-green mica, having implanted on it clear crystals of a mineral resembling some varieties of apatite. The preliminary examination which I have made makes it probable that the mineral is the rare species *Herderite.** The crystals are short, terminated prisms, of from 1^{mm} to 1^{cm} in length and diameter, transparent to translucent. Surfaces smooth and not highly polished, but bright. Luster greasy-vitreous. Streak white. Colorless to faintly yellowish. Hardness 5. Sp. Grav. 3. Crystals orthorhombic, with $I \wedge I = 116°$ (hand goniometer). Among the planes already observed are *I*, *i*-$\bar{i}$, *O*, three macrodomes, one brachydome and three octahedral pyramids. A partial qualitative examination has shown the presence of phosphorus in large quantities. A careful analysis is now under-way by Mr. J. B. Mackintosh, E.M., of the School of Mines, New York City, and will shortly be published, with a full account of the method of occurrence and the general physical characters of the mineral.

This mineral had been previously called topaz, from its resemblance to that species in form and color, and also from the fact of its being found at Stoneham, Maine, in the pockets that yielded the fine crystals of topaz noticed in this Journal by G. F. Kunz (III, xxv, p. 161), but the absence of the characteristic basal cleavage and of the hardness of that mineral led me to make the observations whose results have been given.

[*Note.*—Since the above notice was received from Mr. Hidden, the writer has had placed in his hands, by Professor Brush, a few crystals of the supposed herderite from Maine; these specimens were sent by Mr. Perry. A partial examination of these crystals shows that they approximate closely in form to that given for herderite, so that there can be but little doubt of the correctness of Mr. Hidden's determination. The crystals are prismatically developed in the direction of the brachydiagonal axis (see fig. 454, p. 546, Dana's Syst. Min.) The planes observed are: *O*(001), *i*-$\bar{i}$ (010), *I* (110), $\frac{3}{2}$-$\bar{i}$ (302), 1-$\breve{i}$ (011), 3-$\breve{i}$ (031), 6-$\breve{i}$ (061), $\frac{3}{2}$ (322), 3 (311). Of these planes all occur on herderite except $\frac{3}{2}$-$\bar{i}$, 3-$\breve{i}$, $\frac{3}{2}$. The angles thus far obtained are only approximations, but they serve to show a close correspondence between the Maine phosphate and herderite, for example:—$I \wedge I = 116\frac{1}{2}°$, for herderite $= 115°$ 53′; $O \wedge 1$-$\breve{i} = 156°$ 40′ for herderite $= 156°$ 59′; $O \wedge 3 = 112°$ 40′ for herderite $= 112°$ 35′.—E. S. DANA.]

9. *Analyses of Brazilian Minerals.* (Communicated by Orville A. Derby in a letter dated Rio Janeiro, October 30, 1883).—

* Probably, according to trials by Plattner and Turner, an anhydrous phosphate of alumina and lime with fluorine, orthorhombic with $I \wedge I = 115° 53'$, G. = 2·985. Haidinger, Phil. Mag., IV, i, 1828. J. D. Dana's System Min., p. 546.

GORCEIX gives the following analyses of minerals from Ouro Preto (formerly Villa Rica), in the province of Minas Geraes, Brazil.

Green mica (fuchsite); specific gravity 3·1.

SiO_2	Al_2O_3,Fe_2O_3	Cr_2O_3	MgO	K_2O	Na_2O	Volatile matter.	
46·5	37·2	0·9	0·8	7·9	1·3	4·7	= 99·3

Hydrargillite; specific gravity 2·3.

Al_2O_3	H_2O
65·2	34·8 = 100·00

Wavellite; specific gravity 2·34.

P_2O_5	F	Al_2O_3	CaO	MgO	H_2O
33·0	3·6	36·1	0·3	0·2	26·2 = 99·4

Gorceix calls attention to the large amount of fluorine present in this mineral; it was determined by the St. Claire Deville process.

Pyrophyllite; in greenish white acicular crystals; specific gravity 2·76.

SiO_2	Al_2O_3	FeO	CaO	H_2O
65·3	28·0	1·7	0·4	5·5 = 100·9

In the same article the occurrence of rolled crystals of monazite in the diamond sands of the Jequetinhonha near Diamantina is noted. A similar mineral, found abundantly in sands from Caravellas in the province of Bahia, consists essentially of phosphate of cerium and other rare earths, but differs from the Jequetinhonha specimens in specific gravity (5·01) and in aspect. Its crystalline form could not be determined. By error it is stated to be from the Serra de Quebra Cangalha in the province of Sao Paulo.

10. *Groddeckite: a new mineral of the Chabazite group.*—A new zeolite, to be regarded as a variety of gmelinite, has been recently described by ARZRUNI. It was discovered by him on a single specimen in the museum of the Clausthal Bergakadamie, which was obtained in 1867 from the Franz-August vein at Andreasberg in the Harz. The mineral occurs in small transparent crystals, with vitreous luster, covering calcite crystals. The crystals form combinations of a rhombohedron, a scalenohedron nearly coincident with it, and a hexagonal prism; in habit and angles it is hardly to be distinguished from gmelinite. The hardness is between 3 and 4. An analysis by Broockmann upon 0·0559 gr. gave the following results:

SiO_2	Al_2O_3	Fe_2O_3	CaO	MgO	Na_2O	H_2O
51·2	12·0	7·7	1·1	3·3	[4·5]	20·2 = 100·

A comparison of the above with the percentage composition of gmelinite shows that the new species, or more properly new variety, differs in the substitution of iron sesquioxide for part of the alumina, and of magnesia for part of the lime. The high percentage amount of silica is ascribed to an admixture of quartz, but the material at hand was too scanty to allow of direct proof of this point, or indeed of another and more complete analysis;

the composition consequently must be considered somewhat doubtful. The mineral is named from Dr. A. von Groddeck, Director of the Bergakadamie at Clausthal.—*Z. Kryst.*, viii, 343, 1883.

11. *Mineral Resources of the United States;* by ALBERT WILLIAMS, Jr., Chief of Division of Mining Statistics and Technology. 813 pp. 8vo. Washington, 1883. (U. S. Geological Survey, J. W. Powell, Director).—Mr. Williams has performed a work of much importance in bringing together the extended series of statistics contained in this volume. The magnitude of the results accomplished is especially remarkable in view of the short time taken for the preparation of the volume. The subject is treated in two parts: the first contains statements and statistics in regard to the occurrence and production of the individual mineral products, as coal, petroleum, also iron, copper, lead, zinc, etc.; and, after the metallic products, others as building stones, bricks, clays, fertilizers, salt, borax, sulphur, etc. To the more important of these products chapters of considerable length are given; thus to the subject of coal about one hundred pages are devoted, and similarly of iron, copper, lead, and so on. These larger chapters have been prepared in whole or in part by different contributors, whose past work has given them especial opportunities for acquiring a knowledge of the facts, and they contain a vast amount of valuable information in regard to the amount of each material mined, the cost of production and allied details.

The second part of the volume contains statistics in tabular form, arranged according to States, giving the scientific and popular names of the various ores, minerals, etc., which are now mined, and in a second table those which are known to occur, but which are not at present mined, with a general statement of the localities. These tables do not profess to be complete in their present form, but they form a nucleus which may be expanded and revised as additional facts are collected.

12. *Lehrbuch der Mineralogie;* von DR. GUSTAV TSCHERMAK. III Lieferung, pp. 369-589. Vienna, 1884 (Alfred Hölder).—The two preceding parts of Professor Tschermak's Mineralogy have already been noticed in this Journal (III, xxiii, p. 68; xxiv, p. 232); this third part concludes the work. It contains the description of species, with an appendix devoted to a brief statement of the mineral constituents of meteorites. This portion of the Text Book is very much condensed, too much so to be entirely satisfactory, but it contains much that is fresh, particularly in the way of figures. The work as a whole is an excellent one for the class of students for whom it has been prepared.

III. Astronomy.

1. *Observations upon the Comet Pons-Brooks made at the Observatory of Yale College;* by O. T. Sherman.—The comet Pons-Brooks has been followed, at the Observatory, by the equatorial of 8 inches aperture (presented by E. M. Reed, Esq.) as steadily as circumstances permitted. Up to date (Dec. 10th) thirty-four observations of position have been obtained; six observations yielding the position of the plane of polarization, and six the percentage.

When first seen, Sept. 6, the nebulosity was very faint globular, about .60″ in extent, without definite nucleus; Sept. 25 the comet had suddenly increased in brightness, presenting an oval 95″ by 64″ in extent, light almost equally diffused. No sharp nucleus. The direction of the elongation was about N. 15° W. Sept. 27–Oct. 3, the nucleus continued to grow brighter while the nebulosity remained indistinct. Oct. 16, the coma or condensation around the nucleus became distinct from the outer nebulosity, the direction of elongation being about S. 20° E. Oct. 24–27, there was a suspected tail about 10′ in extent, which had again disappeared by the 30th. Nov. 2d it appeared again as a globular body presenting a sharp nucleus with a coma of about 50″ surrounding it and a nebulosity of about 3′. Nov. 17–19 there was again a suspected development of the tail. Nov. 24 it again presented a form similar to Nov. 2d, but with a fourth distinction in brightness, the dimensions being (approximately), for coma 50″; inner nebulosity 8′; outer nebulosity 10′. On the days immediately following, the distinctness of the third outline was lost and the coma became somewhat radiated. Dec. 3d, the coma appeared somewhat lens-shaped, extending crosswise of the nebulosity about 5′ by 1′. The nebulosity was oblong, about 12′ by 7′.

When examined by a Nicol's prism there appear two positions of maximum intensity, two of minimum but no disappearance. At the time of least intensity the nucleus and a portion of the coma appear deprived of the nebulosity. Under a double image prism the position of equal intensity is 24°·5 removed from the nearer position of maximum-minimum intensity.

2. *Spectroscopic Observations of Comet Pons-Brooks;* by Dr. N. de Konkoly.—The spectrum was very faint and appeared as a diffused nebulous spot of light. After looking in the spectroscope for a long time I saw three bright bands in the spectrum. The bands were all extremely faint and diffused on both sides, the slit being opened rather wide. The second band from the red end was the brightest, the third band was the next brightest, and the least refrangible band near the red portion of the spectrum was the faintest. All three bands ended in a faint point. They were of unequal lengths; the brightest being the longest, the least refrangible next, and the band toward the more refrangible end of the spectrum the shortest.

There were moments in which for one or two seconds at a time the bands were very clear and bright, and at these times they seemed to be much shorter than ordinarily. I should say but one-third the length,—a singular phenomenon which I have never seen on any other occasion.—*Observatory, and Astr. Nachrichten.*

3. *Observations of the Great Comet of* 1882 *made at the U. S. N. Observatory.*—This appendix to the volume of Observations for 1880 is prepared by W. C. Winlock, by whom the larger part of the observations were made. The comet was observed first on the 19th of September by daylight, two days after the perihelion passage. It was last seen on the 4th of April. Eight diagrams are given of the appearance of the head, seven of them as seen in the 26-inch equatorial, showing the disintegration of nucleus, and the gradual separation of the several parts. Observations for the comet's position, differential measurements of the points of light of the nuclei, descriptions of the shape and appearance of the tail, and also of the curious "outer-envelope" or "veil" that extended back toward the sun are all given in detail. H. A. N.

IV. Miscellaneous Scientific Intelligence.

1. *Report of the Superintendent of the U. S. Coast and Geodetic Survey for* 1882. Washington: Government Printing Office. 1883. 4to, 565 pages and 52 plates.—This report has just been received from the Public Printer and is now ready for distribution. As usual the first part of the volume is devoted to a statement of the progress made by the various branches of the Survey in all parts of the country and shows that the work is being prosecuted with vigor and a discriminating regard for the requirements of the country at large, and the necessities of those portions of our extensive coast line which are as yet unsurveyed. This portion closes with detailed statements by the Chiefs of Divisions of the Coast and Geodetic Survey Office of the work done under their direction. Following the report proper of the Superintendent, and the statistical reports from Divisions, are a number of monographs relative to the work. As a collection this is the most important series of papers yet published by the Survey.

The principal geodetic work executed during the year was the measurement of the Yolo Base in California. This primary base line is situated in the Sacramento Valley, about twenty miles west of the city of Sacramento and parallel to the general direction of the Coast Range. As the primary triangulation in California has furnished the largest triangles known to geodesy, so also in this base line we have one of the longest primary bases ever measured. It is 17·49 kilometers in length. Preparations for its measurement had been in progress for several years and for this purpose a new compensating base apparatus was constructed on a principle not new in itself but new as applied to base apparatus. It was made from the designs and under the supervision of Mr. C. A. Schott, Assistant C. and G. Survey. The standard of reference is the iron "Committee Meter" of the United States, one of the original

meters used by the French Committee in 1795. In connection with the construction of the base apparatus the coefficient of expansion of the Committee meter was elaborately redetermined. A detailed account of the steps followed in the determination of the coefficients of expansion and lengths of the various bars used, of the primary and secondary compensation, and of the resulting lengths of the 5-meter standards and measuring bars, is given in Appendix No. 7, by C. A. Schott.

Following this is a paper by Professor Geo. Davidson, Assistant Coast and Geodetic Survey, giving an account of the measurement of the Yolo Base, of which work he had charge. He gives a short history of the base, its selection and survey, and method of marking the ends, and then goes into a more detailed account of the operations immediately connected with the measurement. The working force was twenty-one persons, seven officers and fourteen men. All the operations of measurement were conducted under a large canvas screen moving on wheels and pushed forward by hand. The party, as well as the apparatus, was thus protected from the direct rays of the sun. Daily comparisons were made between the measuring bars and the standard. On the first measurement heavy "kilometer stones" were put down at the end of every 200th bar and on the second measurement comparisons were made. Whenever the field reductions indicated a difference between the two exceeding four millimeters the kilometer was again re-measured. This was, however, not done until the completion of the second measure. Seven kilometers were thus re-measured, but the office reduction showed that the introduction of certain small quantities of which no note could be taken in the hurried work of a field reduction made such corrections to the field differences that the third measurement would nowhere have been necessary. The probable error from all sources combined is 0·0096 meter. Expressed as a vulgar fraction this is $\frac{1}{182000}$ of the length.

The next paper is a reprint, with additions, of the manual on the "Field work of the Triangulation," by R. D. Cutts, Assistant Coast and Geodetic Survey. This paper was originally printed in the Coast Survey Report for 1868, and again printed, with additions, as a separate publication in 1877. In its present form it is still further enlarged and is fully adapted to the needs and requirements of geodetic practice at the present day. The portion of the manual devoted to signals is elaborated in a separate paper on the subject by C. O. Boutelle, Assistant Coast and Geodetic Survey. In this are given a number of tables which will be of value and assistance to those having such signals to erect.

In addition to the one already mentioned there are three other papers from the fertile pen of Mr. Schott. One is a discussion of the results of the line of transcontinental spirit levelling now conducting in connection with the triangulation of the 39th parallel. Beginning with the mean tide level at Sandy Hook, N. J., the line of levels passes Hagerstown, Md., Grafton, W. Va., Mitchell, Ind., and has now progressed seventy miles beyond St. Louis. The

levels were executed by the use of the "geodesic level," a double line being run, not simultaneously but forward and back over short spaces. The small difference between the two and the very slow accumulation of error shows the great accuracy attainable with this instrument. A discussion of the results shows a probable error of only 47 millimeters for the height of the bench mark on the bridge at St. Louis, the linear distance from Sandy Hook being 1784·6 kilometers = 1191·4 statute miles.

The other two papers by Mr. Schott relate to terrestrial magnetism. One is a fifth, and considerably enlarged, edition of his previously published work on the "Secular Variation of the Magnetic Declination in the United States and at some foreign stations." The constant demand for this paper renders necessary its frequent republication and it is so well known that more detailed mention of it need not here be made. Following this is a paper giving magnetic declination for the epoch 1885·0 at more than 2300 stations in and on the confines of the United States and Alaska. It is accompanied by isogonic charts for the U. S. and Alaska. The last chart of this character published by the Survey appears in the report for 1876 and represents the distribution of terrestrial magnetism in the United States for the epoch of 1875·0.

Following the last named paper is one by Professor J. E. Hilgard giving the results for magnetic declination, dip and intensity as obtained between 1871 and 1876, through aid of a portion of the Bache Fund for scientific research, so applied by the National Academy of Sciences, and of which work Professor Hilgard had the direction. One hundred and forty stations were occupied. They represent twenty-six States and Territories, from Vermont to Florida and Wyoming, and also Canada and New Brunswick. Observations for declination were made at 141, for dip at 94, and for intensity at 93 stations. Full abstracts of the results are given and the values were used in the preparation of the isogonic charts of 1875·0 and of 1885·0.

Five papers devoted to the consideration of hydrographic questions come next. Of these the most important is a "Discussion of the tides of the Pacific Coast of the United States," by Wm. Ferrell, whose contributions to meteorology and tidal researches have become everywhere known. In this paper Mr. Ferrell discusses the hourly results for a period of three years at the stations Port Townsend, Astoria and San Diego, and his deductions form a valuable addition to the data previously in hand for predicting the tides on that coast. The other papers are a "Comparison of the Survey of Delaware River of 1819, between Pettys and Tinicum Islands, with more recent surveys," by H. L. Marindin, Assistant Coast and Geodetic Survey; a "Study of the effect of river bends on the lower Mississippi," by Henry Mitchell, Assistant Coast and Geodetic Survey; a "Report on the Siemens' Electrical Deep Sea Thermometer," by Commander J. R. Bartlett, U.S.N., Assistant C. and G. Survey, commanding C. and G. Survey Steamer Blake, and on "Recent Deep Soundings off the Atlantic Coast of the United States," by Lieut. J. E. Pillsbury, U.S.N., Assistant C. and G. Survey, Acting Hydrographic Inspector.

Succeeding these are a paper by Professor Geo. Davidson giving an account of the total solar eclipse of January 11, 1880, as observed by him at Mt. Santa Lucia, California, and a "New Reduction of Lacaille's Observations of Fundamental Stars in the Southern Heavens" (between 1749 and 1757), the computations of which were made by Dr. C. R. Powalky, under direction of the National Academy and at the expense of the Bache Fund. This paper will find publication in the Bache Fund Memoirs, but is here published to give earlier appearance to so important a work. This re-computation, taking account of and correcting the many imperfections of Lacaille's instruments, will give the most valuable data for the determination of the proper motions of many of the southern stars.

This paper is followed by a report on a "Conference on Gravity Determinations, held at Washington, D. C., in May, 1882," in which Hilgard, Herschell, Peirce, Newcomb, Davidson and Schott participated. The present state of the science was discussed and opinions expressed as to the method of conducting gravity observations and the sense of the conference was formulated in distinct categorical statements and conclusions which are now published.

Finally, the Report closes with a tribute to the memory of the late Carlile P. Patterson, LL.D., fourth Superintendent of the Survey.

2. *Annals of Mathematics, pure and applied;* edited by ORMOND STONE, Professor of Astronomy, and WILLIAM M. THORNTON, Professor of Engineering, both of the University of Virginia.—This new Journal has its office of publication at the University of Virginia. It is announced as "the successor of the Analyst, which has been edited for the past ten years by Mr. J. E. Hendricks, and which is now discontinued by him on account of impaired health."

The Annals of Mathematics will appear every other month, commencing with February 1st, in a small quarto form, and each number will contain at least twenty-four pages. It is designed to be a medium of communication and publication for teachers and students of mathematics. The purpose of the editors will be to guide and encourage the study of mathematics, pure and applied, in all its branches; to stimulate independent mathematical investigation by offering prompt publication of its results; to report the more important advances in mathematical discovery; and, to register the more valuable additions to mathematical literature. The subscription price is two dollars per annum. The Journal should have the support of all who are interested in the advance of mathematical studies and in methods of instruction.

Chemistry: Inorganic and Organic, with experiments, by Charles Loudon Bloxam. From the fifth and revised English edition. 738 pp. 8vo. Philadelphia, 1883 (Henry C. Leas' Son & Co.) This well known and standard work has now passed to its fifth edition.

A Manual of Chemistry, Physical and Inorganic, by Henry Watts, B.A., F.R.S., 595 pp. 8vo. Philadelphia, 1884 (P. Blakiston, Son & Co.) A new work by the author of the Dictionary of Chemistry; it differs from most recent elementary works in that it gives so much space to Physics.